

THE
AMERICAN
MEDICAL RECORDER.

VOL. III.

JULY, 1820.

[No. III.]

MERCURIAL OINTMENT IN ERYSIPELAS.

On the use of Mercurial Ointment in the local treatment of Erysipelas. By A. T. Dean, M. D. of Chambersburg, (Pa.) in a letter addressed to Dr. M'Dowell of Baltimore.

My engagements, and a delicate state of health, have heretofore prevented me from answering your letter. I shall now, however, in compliance with your request, briefly state to you the result of my experience with the mercurial ointment in the local treatment of erysipelas.

In the spring of 1817, I was requested to visit a respectable young lady of this place, who, for a number of years, has been subject to repeated and violent attacks of the above disease. She had been suddenly seized with chilliness, accompanied with great anxiety and oppression about the præcordia, nausea, vomiting, and other symptoms indicative of gastric derangement. Her pulse, which, at first, was small and frequent, upon the reaction of the system, became full, corded, and tense, or what

VOL. III.—Q q

has been usually denominated a synocha pulse. She complained of the most excruciating pain in her head, and although the erysipelatous eruption had located itself upon the lower extremities, a considerable degree of coma supervened upon the third day. These symptoms continued to increase in violence, and together with the intolerable heat, itching, and burning pain, arising from the local affection, rendered the situation of the patient peculiarly distressing.

As the inflammatory symptoms appeared to constitute the most prominent feature of the present case, the patient was freely bled, purged, and took such other medicines as were calculated, with these means, to effect a reduction of arterial action. The bleeding was repeated from the arm and temporal artery, as often as the urgency of the symptoms seemed to require, and in such quantities as were proportioned to the violent grade of excitement which pervaded the system. A pretty constant catharsis was also kept up, not only with a view of co-operating with the lancet in lessening the activity of the pulse, but in order to relieve the affection of the head upon the principles of revulsion. With the latter intention, blisters were likewise applied to the back of the neck and the fore part of the head.

By these general remedies, and a rigid adherence to the antiphlogistic plan of treatment, the violence of fever was somewhat abated, and the comatose affection of the head considerably relieved, but in other respects the patient was no better. The local affection continued to spread, and was attended with an increase of all those distressing sensations of heat, &c. which are so peculiarly characteristic of this species of eruption. Nor did the various cooling emollients, and soothing applications, that have been recommended by Cullen, Vogel, Buserius, and other respectable physicians, and which were used in conjunction with the above constitutional means, afford the least relief to the sufferings of the patient. On the contrary, they appeared rather to augment the violence of the inflammation. Even blisters, which, upon other occasions, I had resorted to with the most decisive effect, failed in arresting the progress of the complaint.

Under these circumstances, the ung. hydrarg. fort. was freely

rubbed into the parts occupied by the erysipelatous inflammation, and the limbs afterwards enveloped in fine linen cloths, upon which some of the ointment had been previously spread. This process was repeated twice, or three times in the course of the day. A few hours after the first application of the ointment, the patient expressed much relief from the burning pain, heat and itching in her limbs. Upon a farther continuance of the remedy, the eruption ceased to spread, and assumed a more favourable aspect; so that, by the morning of the third day from its first application, the inflammation had entirely disappeared, and with it the general irritation of the system.

The same young lady has since had several attacks of erysipelas upon her face, but of a character so local as to yield to the mercurial ointment alone, or at least, when assisted by a gentle cathartic. This patient has assured me, that of all the applications she ever resorted to, when afflicted with this complaint, none have afforded her such prompt and effectual relief as the mercurial ointment. Its effects, in some cases, have been so immediate and complete as to put a check to the disease in the short period of twenty-four hours.

In the spring of the same year, I was requested to visit, in consultation with a medical gentleman of this place, a young man of the name of Ross, who had been confined to bed for some time before I saw him with a fever of the typhoid kind, attended with an extensive erysipelatous affection of the lower extremities. He had been judiciously treated. The attending physician had administered such general remedies as were indicated by the particular state of the system, besides resorting to the most approved local applications. Among the latter, if I recollect right, blisters had also been used. The disease notwithstanding continued to spread, and by the time I saw the patient, the inflammation had extended itself over the greater part of the legs and thighs. I mentioned to the attending physician the efficacy of the ung. hydrarg. upon such occasions, and recommended its immediate application. It was accordingly applied, and, in a short time, evinced its powers by allaying the troublesome burning, and imparting to the patient an agreeable sensation

of coolness, which was soon afterwards followed by an entire removal of the disease.

Not long after this, I had again the pleasure of witnessing the good effects of the mercurial ointment in the cure of erysipelas. A married lady of this place, about the period of the cessation of her menses, was seized with an erythematous efflorescence which extended itself over the neck, breast, and superior parts of the body, encircling the latter in such a manner as to form the *zona ignea* of medical writers. It was accompanied with a degree of heat and smarting almost insupportable to the patient; or, to use her own expression, "she felt as though she were encompassed by a flame of fire." The excitement of the vascular system, which was entirely sympathetic, bore no proportion to the extent and severity of the cuticular inflammation. Nor was the head in the least affected. This case, you will at once perceive, was of that species of erysipelas, which Doctor Cullen has designated, in his *Nosology*, by the name of erythema, and which, he supposes, to arise from the operation of local causes. It is to this variety of the disease, I believe, that the ung. hydrarg. is in an especial manner adapted.

With this patient, previously to the use of the ointment, I tried in succession a mixture of the extract of lead and laudanum, the camphorated spirits, and a solution of sal ammoniac. These washes, instead of abating, evidently increased the heat and inflammation of the parts, and I was finally compelled to have recourse to the ointment in order to check the progress of the disease, after having prolonged the sufferings of my patient, partly with the view of ascertaining the comparative merits of the several preceding remedies.

This patient was also bled, purged, &c. but as my principal object is to state to you the powers of the mercurial ointment as a topical remedy, I shall not occupy your time in giving a detailed account of the general treatment pursued in the present case, and will now observe, once for all, that I do not wish to be understood as relying, in any instance, upon the mercurial ointment, to the exclusion of those constitutional means, which the particular state of the system may render indispensably necessary.

In this country, erysipelas is, for the most part, a disease of an inflammatory character, and sometimes of so violent a grade, as to require the most prompt and copious detractions of blood, along with other modes of depletion, in order to subdue it. On the contrary, we occasionally find it connected with a typhous state of the system, which calls for an opposite mode of treatment from that just mentioned. But whether the disease assumes the stenic, or astenic form, I would be inclined to repose confidence in the mercurial as a topical remedy, upon the supposition of its removing the complaint by the substitution of its own peculiar action in the room of the diseased one already existing in the skin. That no two actions, of different degrees of force, can possibly exist in any one part of the system, at the same time, is a position so generally admitted, and supported by such a variety of facts, as to render it superfluous in me to say any thing farther in confirmation of it. If, therefore, the inflammatory excitement of the skin, in the more violent cases, be so far reduced by depletion as to fall within the sphere of mercurial action, the ointment, I apprehend, will be found adequate to the removal of both varieties of the disease. Independently, however, of these speculative notions with respect to the *modus operandi* of the mercurial ointment, I am led to the same conclusion from its beneficial effects in the case of Ross, which, as before stated, was connected with an astenic condition of the system.

I have since used the ung. hydrarg. in a number of cases, in all of which, it has been productive of the most beneficial effects. Indeed, among the various local remedies that have been recommended in this disease, and which I have had an opportunity of trying, there are none, I am inclined to think, superior to the strong mercurial ointment. The trials which have been made with it in this part of the country, have also obtained for the ointment the confidence of some of our most respectable physicians. The medical gentleman, who attended in the case of Ross, has since had frequent opportunities of testing its efficacy, and he is still farther convinced of its pre-eminence over the local remedies now in general practice.

In one instance only has it proved unsuccessful in his hands.

This was in the case of a child which, in consequence of the sudden disappearance of the erysipelalous efflorescence from its face, fell into convulsions and died. The Doctor was at a loss to know, whether this retrocession of the complaint was owing to the ointment, or some other unknown cause. As this effect sometimes occurs where no external applications have been made, and often without any assignable cause, it is probable, that the mercurial ointment had no particular agency in bringing it about.

Another instance of the failure of the mercurial ointment, occurred in the practice of Doctor Little of Mercersburg, the particulars of which I have never learned. The Doctor has been in the habit of using the ointment in erysipelas for two or three years back, and with the exception of the case alluded to, with uniform success. Indeed, these are the only two cases in which I have heard of the failure of the ointment.

A pupil of mine, who graduated last spring at the medical college of Philadelphia, made erysipelas the subject of his inaugural dissertation, and among other local remedies, recommended the mercurial ointment as one of superior efficacy, in consequence of which, it was tried in a case which occurred in the hospital at that time, and he informs me, with its usual good effects.

The ineffectual application of blisters in two of the preceding cases, which were afterwards cured by the mercurial ointment, would seem to point out the latter as the most efficient remedy. Their comparative powers, however, I shall leave to be determined by a more enlarged experience, and only add, that the mercurial ointment can be rubbed on parts, (the face for instance,) to which the application of a blister would be extremely inconvenient, if not impracticable, and that it will, at all times, be a remedy less repugnant to the feelings of the patient.

From the beneficial effects of the mercurial ointment in other cutaneous affections, it is matter of surprise, that analogy had not led to its use in erysipelas. It is probable, however, that the prejudices, which have existed from the days of Galen down to the present time, against the propriety of making oleaginous applications in erysipelas, has deterred practitioners from the use of the ointment in this disease. Doctor Darwin has given us

some theoretical speculations in explanation of the supposed injurious tendency of unctuous applications in this disease, but as the Doctor's hypothesis rests upon a position, which experience now teaches us to be unfounded, his reasoning must of course be entirely nugatory.

A slight affection of the mouth has sometimes arisen from the use of the ointment in erysipelas, but, this is by no means essential to the success of the remedy. A salutary impression is always made upon the complaint before the ointment has had time to extend its influence to the general system, or at least, before there is any evidence of its having produced a constitutional operation. Its good effects therefore, I apprehend, are principally to be ascribed to its local action.

Other mercurial preparations will perhaps be found to answer in this disease as well as the ung. hydrarg. Of these the ung. nit. rub. or the muriate of mercury, either in the form of solution or unguent, might be tried.* I can, however, say nothing in their favour from experience.

While upon the subject of erysipelas, I would observe, that like some other affections of the skin, it appears to be frequently connected with, if not entirely dependent upon a deranged state of the stomach and bowels. The bitter taste in the mouth, the foul tongue, headach, nausea, vomiting, and other symptoms of gastric distress with which it is usually ushered in, unquestionably point it out as a complaint sometimes having its primary seat in the stomach, from which the morbid impressions are afterwards propagated, forming a train of diseased associations ultimately involving the skin. Many other of the eruptive diseases, both in children and adults, we also find, are closely connected with the state of the primæ viæ, but in none is this connection more

* The muriate of mercury dissolved in water, has been employed in erysipelas, by Dr. Schott of this city, for about five years past, with much success. More than a year ago, he informed the editor of its efficacy, when externally applied in this disease. Since that period the editor has had two cases, in which it proved a most beneficial application. The strength of the solution used by Dr. S. is one grain of the muriate to one ounce of water.

EDITOR.

conspicuous than in erysipelas. Anxiety, pain in the stomach, eructations, nausea, and bilious vomitings, says Buserius, are among the symptoms preceding an attack of the latter disease. Schroeder also observes, "*Accedit non rare ciborum fastidium, sapor amarus, nausea, conatus vomendi, aliquando etiam vomitus.*" Such is the dependence of erysipelas upon the condition of the *primæ viæ*, that according to the last mentioned author, the disease is often entirely removed by the evacuation of the bilious and irritating matter from the stomach and bowels. In confirmation of the same opinion, we might refer to the writings of Hippocrates, Galen, Lieutaud, Tissot, and others. Among physicians of later date, who have advocated the same doctrine, we may also mention the names of Quarin, Vogel, and Desault. The latter particularly, in his *Surgical Journal*, has recorded some striking cases, which are strongly corroborative of the gastric origin of this disease.

If, then, the preceding remarks respecting the pathology of erysipelas be well founded, they at once suggest the importance of emetics and cathartics in the general treatment of the complaint; and indeed, experience teaches us, that their frequent exhibition is indispensably requisite. Independently of the evacuations they produce, they leave upon the stomach and bowels an impression, which is subversive of their diseased action, and also of that of other parts of the system, sympathetically connected with the morbid condition of these organs.

There is an obstinate and troublesome disease of the ears, for which I have found the mercurial ointment an excellent remedy. I do not recollect to have met with a description of this complaint in any medical or surgical work. It is seated in the *meatus auditorius*, is attended with a severe and painful itching, a constant beating and ringing in the ears, and at times, with a thin, acrid discharge, that excoriates and inflames the parts over which it flows. This discharge appears to proceed from a number of small ulcers which are spread over the surface of the *meatus*, and probably over the *tympanum* itself. The ulcers will some-

times of themselves heal up, and remain in that state for weeks, or even months, leaving behind no other indication of disease than an occasional itching; they will then gather, again break out, and continue to discharge for several months, to the great annoyance and distress of the patient. During the period of gathering, as it is familiarly termed by the patient, there is an extreme degree of tenderness for some distance round about the meatus externus, which forbids even the slightest touch.

I do not know that this complaint has ever terminated in deafness, but in some cases, which have fallen under my observation, the hearing has been considerably impaired. I have known it alternate in one person with a painful heat and itching of the eyes. When in these organs, the ears were entirely free from complaint, and on the contrary, when the latter were diseased, the former were perfectly well. Before using the ointment, I had been very much baffled in the treatment of this complaint. I had tried blistering behind the ear, and resorted to various stimulating and astringent injections, consisting of sulph. zinci, superacetas plumbi, sulph. cupri, &c. but all to no purpose. The ointment is the only remedy, which I have found to have any control over this disease. By means of it, I have succeeded in curing several obstinate cases, which resisted every other mode of treatment. If it does not effectually eradicate the complaint, it will at least suspend, or cure it for a time.

I have observed in the last number of the Medical Recorder, that the surgeon aurist to the Prince Regent, J. H. Curtis, Esq. has successfully treated with the argentum nitratum, the puriform discharge from the ears. This complaint in the ear, I consider less difficult to manage by the ordinary remedies, than the ulcerated affection of which I have just been treating. I have succeeded in curing it by repeated blistering, and a perseverance in the use of astringent injections.

On Erroneous Notions of Duty in Insane People. By Ezra Gillingham, M. D. of Baltimore.

CONSCIENCE is a quality of the mind about which there has been much misconception. It being a medium through which the sovereign of the soul influences the voluntary conduct of his rational creatures, difficulties have arisen in regard to its certainty. If it were granted to be a principle divine and infallible, it would evidently be absurd to speak of any influence upon the correctness of its operation. There are circumstances which may prevent the mind from understanding the directions of an unerring guide, but they never can regulate its decisions. But if conscience is subject to delusion in common with judgment, memory and imagination, it must be a natural quality of the mind, and liable to be perverted by causes which operate upon the human intellect.

Our excellent Dr. Rush supposes that there is another principle independent of conscience, by which we receive information of our duty. This he terms a moral faculty. But there are objections to the existence of any such faculty as he describes, and his attempts to show its character are far from establishing its reality. His assumption of its existence is hypothetical; he confounds it with conscience; and supposes it to be the same principle spoken of in the 1st. chap. of St. John. It would be improper in this essay, to discuss a point which would lead so immediately into theology, and I shall only observe, that I believe the generality of christians consider the text to which Dr. Rush alludes, to refer to the Saviour of the world. These objections derive weight from the reflection that neither metaphysicians nor religious writers, acknowledge the existence of any natural moral principle except conscience. When conscience is not directed by the operations of the human soul, it must be by the grace of a superior Being.

Conscience, then, is liable to be affected by causes which disorder the human mind, and is the only natural principle by whose

instrumentality we receive approval for good or checks for evil actions. There can be no ground for asserting its infallibility. Just as is the education, so in general is the conscience. The Turk, the Jew, the Brahmin, all pursue things conscientiously which we abhor; and indeed whatever a man is firmly convinced to be right, he will believe it his duty to perform. "Conscience," says a celebrated writer, "comes from *conscire*, and is that knowledge which ariseth in man's heart, from what agreeth, contradicteth or is contrary to any thing believed by him, whereby he becomes conscious to himself that he transgresseth by doing that which he is persuaded he ought not to do." A disordered conscience must therefore be the effect of a disordered judgment, and upon this principle there is no difficulty in explaining all those extravagancies of conception which sometimes attend the commencement of insanity.

False notions of duty sometimes arise from the state of the intellect, and sometimes from the condition of the body. The first cause however has acted most frequently and most extensively.

It is interesting to observe the wild conduct which has resulted from a disordered sense of duty in various periods of time. History affords many cases to show how easily a man's judgment may be perverted until his conduct approaches that of madmen. Delusions are associated, and fondly introduce one another; and when a man suffers his mind to become once their victim, they soon begin to lead him towards a precipice, a fall over which will either produce destruction or misery and decrepitude through life.

The consciences of men have often become perverted by the influence of fanatical teachers until there have been very strong indications of insanity. The anabaptists of Munster, the fifth monarchy men of England, and those wretches in our own country who with the view of performing the supposed duty of extirpating the heathens, murdered the Indians in Lancaster jail, are instances of this kind. A very striking example of this epidemic approach towards insanity is to be found in the early history of the monks. When Anthony commenced the anchorite

system, I have great suspicion that he was insane. The contagion however spread like lightning, and all Europe and Asia were filled with men striving to outdo each other in extravagance. They wandered naked in deserts; they lived in dens and among rocks and precipices; they suffered their bodies to become filthy, and wore their hair and nails long, like wild beasts, whose mode of living they appeared desirous to usurp. Finally, Simeon, one of those called Stylites, excelled them all in absurdity by spending a large portion of his life upon a pillar which he had raised gradually from 9 to 60 feet in height.*

* These melancholy instances of human delusion are all explicable by supposing a perverted judgment, whose absurdities are made sacred by conscience and stamped with the imposing character of duties. Perhaps in most cases perversions of the judgment which lead to those false notions of duty arise from the power of passion. Sometimes when men become eager to possess reputation they are blinded, and their ambition exceedingly preposterous. They seem capable of becoming competitors with every portion of human nature. There is no object, however great or however contemptible, in which they cannot discover some source of applause. Those who have attempted to imitate our Saviour and Elijah in their 40 days fast may have been of this number. One case is related in which a young man supposed that if he completed his fast, he would become some extraordinary being by miraculous agency. Here was the same principle in action which urged Herostratus to lay his torch to the temple of Ephesus. It would not be proper to apply this explanation to the cases of extraordinary notions of duty in which the insanity was confirmed. Thus James Hadfield, whose story at his trial for shooting at George III. was so eloquently told by Lord Erskine, imagined that it was necessary to suffer death for the salvation of mankind. His delusion evidently arose from an injury of the brain.†

* See Gibbon or Mosheim. Poor Simeon however had some consolation in his aerial residence, for his extraordinary sanctity drew thousands around him, and even the Emperor Theodosius sought his counsel.

† Erskine's speech at the trial of Hadfield.

The transition from sanity to madness when the disorder is accompanied with false notions of duty, is apt to be by slow degrees. It is very affecting to view the state of a mind while progressing towards insanity, but still possessing self-control. A presentiment often attends it of the disorder which is approaching. The cloud which is about to shroud it is awful, and it struggles to turn aside the darkness. A man in these circumstances will often mention the duty which he feels bound to perform. He will anxiously seek for sympathy. He feels something rankling in his bosom which he cannot pluck out, and tears are wrung from him at the unavailing effort. The struggle is greater because all his views are preparative to action, and because conscience, severe, and implacable, speaks to him in tones of thunder for revolt.

A case occurred in this city about two and a half years since, which, if I can properly interpret it, will serve as an example of this struggle against the approaches of insanity. A stranger apparently about 45 years of age, not making any particular profession of religion, was observed to attend very constantly the devotions of one of our religious societies. He was serious and modest in his deportment, and nothing very striking was observed in his character by the congregation. After several months had elapsed a minister delivered to a large concourse of people, a forcible discourse on the duty of relinquishing all selfish views, and yielding up the whole soul to perform the will of God. When the minister had concluded, the man stood up and made a public acknowledgment of his selfishness. It seems he had notions of duty which his reason had told him to reject, but now being resigned to part with all and to perform at hazard what he imagined to be his duty, he was in a moment completely maniacal. The next morning, he rushed into an assembly at the same house under extreme agitation. He proceeded with eagerness to take the seat of the ministers, and on some attempt being made to induce him to retire, he declared that the people were ignorant that they were the children of Israel, and that he was a prophet. He continued for some months disordered, and then left the city. Had not the pathetic discourse of the minister acted thus power-

fully on the feelings of the stranger, he probably might have continued to struggle against his imaginary duties, until other trains of thought had dissipated them.

Although the minds of those unfortunate beings are far from the want of acuteness in reasoning, yet their notions are seldom derived from general principles, but supposed to be immediately received from Heaven. The uneasiness therefore which is the consequence of omission is not only to be completely removed by the act, but rewards are to follow. Even in those cases in which murder was performed from a sense of duty, an entire calmness sometimes succeeded the dreadful uneasiness which the thought of the act occasioned.* Reason is seldom capable of overcoming these false notions of duty, because the minds of their possessors are apt to be extremely distrustful of any thing which they think tends to obstruct the Divine will.

Dr. Mead, in the commencement of his work on the Influence of the Sun and Moon upon Human Bodies, introduces the following cautionary precept from Plato: "Let none unskilled in geometry enter here." With peculiar propriety may a caution of a similar nature be made to those who are unacquainted with the operations of the human mind, that they beware of undertaking the removal of false impressions of duty. The relief of these cases forms one of the most important offices which belong to us. The qualifications of the physician who should attend them ought to be peculiar. He should not only be acquainted with the operations of the human mind in its ordinary state, but also in that condition in which it has undergone the powerful impressions of religion. For it is impossible that one should be able to distinguish the sound, from the unhealthy feelings of the religious, while he is a stranger to the action of a superior power upon the human soul.

After stating how high a character is required in him who attempts the relief of these cases, it will become me to be modest in offering directions which are more the result of reflection than experience.

* Vide Crichton on Mental Derangement, vol. 2.

In all instances where the mind is to be regulated and brought into health, by the force of doctrine and example, the conduct of the physician must depend very much on his good sense. If he attempts to form rules from which there must be no deviation, he will probably do very little. Each case may have something peculiar in its treatment, and he must search among the stores of his own mind for the remedy.

The longer the mind of the diseased person is engaged in contemplating his duties, the more their number will increase. In a great number of cases, therefore, it will be necessary to divert if possible his mind from its accustomed reveries. This must be by cautious reasoning. It would often be absurd to attempt to excite the mind into mirth. If the disordered person should consider himself bound to be devoted entirely to solitude, prayer, and contemplation, it may be proper to draw him slowly into the study of nature as connected with theology. Too sudden a transition from his ordinary course of reading might alarm and disgust, because he would consider it to be parting with what he would believe to be the only valuable employment of life: I mean devotion. The study of natural theology is besides a suitable science to commence with, as its tendency is to create an enthusiasm in the pursuit after knowledge without any injury to genuine religious feeling.

It would not be advisable to encourage the patient to

“Lay schemes for wealth, or power, or fame, the wish
Of nobler minds, and push them night and day.”

Although this would probably bring their imaginations temporarily under government, yet there is no doubt that when their minds should be elevated by any thing so contrary to their accustomed feelings as ambition or the love of wealth, a great degree of depression would ensue. Struck with terror when they discover how strongly their affections are engaged in pursuit of what they have hitherto carefully shunned, they are apt to throw all aside and rush to their former delusions.

There are instances in which it would probably be proper to suffer the patient to carry his feelings to their full extent. When

he had completely performed the imaginary duty, his conscience would become satisfied and his mind would be in a proper condition to be invigorated and restored. A young man in the neighbourhood of this city, deeply affected with the subject of religion, began to act very strangely and to have extraordinary notions of duty. He was kept somewhat in restraint; his religious notions were repressed, and his devotional exercises not much encouraged. He grew worse, and his imagination became continually more wild and heated. In this state he went to a camp meeting; joined in the exercises with ardour; and after completely exhausting himself, became calm and tranquil. He continued so while he remained with the society which held the meeting, which was several months, but of his present state I am not well informed.

The medicinal treatment of these cases must be left to the judgment of the physician. There are no specifics for madness. The physician must use his medicines when the body demands their employment.

The unfortunate subject of this disorder, who is always liable to be wrapped up in the blackest folds of despair, has strong claims upon our sympathy; especially as there are many who would rather ridicule the garment, than wash out its gloomy dye. He commences his religious career with all around him possessing the smile of spring. The cloud of summer, the withering air of autumn, gradually spread over his soul, and the ice of winter chills and fetters him. In this state darkness is his clothing, and we cannot afford him relief. The crowd of imagination, which hover around him, and which, to use the imagery of Collins, "lap the blood of sorrow," elude our grasp. Until then he is in our power, and no one should neglect the means of relief, for he who now rejoices in his strength, may soon become a similar victim, pitied by the good and spurned by the cruel.

4th mo. 10, 1820.

On Hydrops Capitis Infantum. By J. C. Rousseau, M. D. of Philadelphia.

PHILOSOPHICAL researches have seldom a single object in view, but the steps of the inquirer ought to be directed by the principal motives that invite the inquiry.

In medical science, curiosity may not altogether be laid aside, although it should become only a secondary object and of minor importance, when the sublime power of healing comes under consideration.

The animal functions, however, as well as the other operations of nature, are so intimately connected with, and so much depending on one another, that it is extremely difficult to judge of the motives of the inquirer, and still more so, to foresee the result of his inquiries.

Newton blowing soap bubbles, and Franklin raising a kite, were far from giving room, to suppose the result of their future discoveries. They were undoubtedly ridiculed and laughed at by many, if we judge by the little interest that the apparently trifling occupations and labours of the votaries of the healing art create at this present day.

The first steps to the most useful discoveries have subjected, and will, I am fearful, expose at all times, the devoted philosopher to the ridicule of the multitude, and the powerful opposition of the envy and jealousy of his cotemporaries: but happy he who may live to see his labours crowned by the gratitude of those who have been, or may anticipate to be benefited by them.

Although it be generally deemed, and it may perhaps be sometimes hazardous to deviate from a beaten path, especially amongst the hidden dangers of the field of science, if, after ages, the safest of the roads pointed out is yet so unsafe as to permit but few to escape through it, shall we call rashness that spirit of philanthropy which, stimulated by the impulse of a love, and a strong desire of benefiting mankind, embraces every opportunity to borrow the safe light of experience for exploring a new road, and by the forcible evidence of those who have safely and successfully

passed through it, adds to the desire, not only the hopes, but a firm confidence of success?

This pleasing stimulus, and the want of success attending the usual mode of cure, together with my views of the origin, causes and effect of the disease I propose to inquire into, induced me, as far back as the year 1807, to adopt a different method of treatment. It having since crowned my sanguine expectation, with all the success that could have been wished for, I am extremely glad to be fully supported in declaring that, from the few, *extremely few*, instances of disappointment I met with, this branch of practice of our profession, has acquired the most beneficial improvement.

The difficulty of ascertaining the existence of this disease in its incipient state is extreme; not only from the want of proper information, but from the structure of the organ affected, and an identity of symptoms common to other diseases. It is only when the disorder has progressed for some time, that we are apprised of its existence: the softness of the bones, and the feeble adhesion of the sutures of the head of the infant, permit the enlargement of this organ; it assumes a peculiar shape, and the progress of this malady is always in proportion to the partial or general degree of debility of the subject.

Led to believe, and persuaded by experience, that all dropsical affections proceed from a partial or general state of debility; convinced by a series of numerous experiments which I published many years ago; that no hopes can be entertained of transmitting remedies *ab externo ad internum* by applications on the surface of the body; considering the long course that remedies received into the stomach have to take, and the changes they must unavoidably suffer, before they can reach the cerebral organ and its dependencies; taught to admire the intimate connection that exists between all the parts composing the animal frame,—a connection more strongly marked between some than others, and particularly between the brain and the stomach,—I was induced to improve this opportunity offered by the properties of our organization, to attack a malady, almost generally regarded as incurable.

The several opinions entertained and manifested by some emi-

dent physicians, respecting the primary cause of hydrocephalus, may perhaps be of some importance, in the prevention of this disease, or for its cure in an incipient state; but the collection of water having once taken place and continuing to accumulate, it seems of very little moment to attribute it to inflammation, for inflammation itself is but the effect of some other causes yet to be ascertained. In fact, inflammation appears to us to be nothing more than the natural reaction intended to oppose a powerful stimulus; but it is not always an inevitable consequence, since we find some of the most powerful stimuli produce an adequate excitement without this result, whereas in all cases the degree of debility is always proportionate to the intensity of the cause that has produced the excitement.

May not a natural weakness exist in some part of the constitution, without having been affected by unnatural agents?

It is in my opinion no less erroneous to attribute this disease, to injuries externally done to the head, for the following reasons; that the head of children being in general the part of their frame that is the most exposed, and receives the most frequently blows and falls, we should see the disease much more frequent than it is. Further, many infants have been born with this disease, and a number have died in utero with symptoms identifying its existence.

I have always remarked, from all those that fell under my observation, that it was the result of a vicious constitution, or of causes more remote than is generally supposed. It is certain and evident that the head of those predisposed to hydrocephalus, is of a peculiar shape, which is seldom, if ever, entirely altered, after the disappearance of the disease.

Leaving conjectures aside, to resort to experience, we shall find this disease to be merely local, and affecting exclusively the head, until, from the want of regular performance of the cerebral functions, other parts of the organization become successively affected and diseased. In this ultimate state of things, the hopes of success in attempting a cure, decrease in proportion to the injury received by other organs the most necessary to life, the mutual dependence being equal on all sides.

From the intimate and subordinate connection of the vital organs, and the views I entertained of the primary cause of this disease, as stated above, it was consequential to admit, that the grand desideratum to obtain a cure was to repair the tone of the vessels and enable them to perform their healthy functions: for it might be rationally supposed that the serous collection enlarging the head was entirely owing to the laxity of the texture of the vessels suffering to escape, the fluids that they ought to retain, and to a want of natural vigor in the absorbents which disqualified them for taking up this superabundant effusion, which by compressing the medullary organ, and opposing its necessary influence, disturbed the whole system.

Although it be worthy of attention to remark that infants born with hydrocephalic heads, evinced at their birth all the marks of perfect growth and vigour, we must not conclude that the continuance of the disease, after birth, might be attended with as little detriment to their new existence.

To the period of its birth the fœtus receives from its mother, to whom it is attached by the placenta, the materials necessary to the growth and support of his yet imperfect organization. Like the root of the vegetable, this organ performs functions which, after birth, are to be replaced by others of a more complicated nature, and entrusted to organs of a superior structure, for the purpose of imparting to the new being that excellence of life which characterises its existence, and is unknown in the womb, where it received its primary rudiments.

To this period, the brain appears to be of little or no importance; and if I be indulged in declaring my opinion, I have strong reasons to make me believe that, in utero, the brain has no more influence upon the animal organization, nor any more functions to perform, than the pulmonary organs.

The instances of animals born without heads, are not few, and their full growth to the period of parturition sufficiently confirms the foregoing opinion, if no other proof could be given.

I was, some years ago, a witness to a case of embryotomy necessitated by the malconformation of the pelvis, where the fœtus,

after being deprived of its brain emitted cries, and struggled with all his limbs for more than half an hour after its extraction.

The motions of the fœtus in the womb are merely spasmodic; and after birth, we see it totally motionless, until it has received the influence of the air upon the lungs.

Were it possessed of sensibility before its birth, it would be hard to conceive how it could bear all the hardships of its passage into the world, particularly in cases of preternatural labour, and instrumental delivery.

The natural term of gestation being completed, and a superior degree of perfection acquired, a new mode of existence becomes necessary, and the assistance of new organs indispensable. From a combination of causes and effects, yet far from being unravelled by the human mind, results then the stimulus necessary to cause the contraction of the womb, and operate the expulsion of its contents.

Several systems become then indispensable in the warm-blooded animals, for the continuance of their yet imperfect existence, namely, the sanguiferous, the pulmonary, the gastric and the medullary or cerebral. Although intimately connected together, they have nevertheless very different functions to perform, and lend each other a mutual assistance.

The sanguiferous system, the first and most essential part of the rudiments of the animal, destined to carry to the most distant parts of the economy the materials necessary to its growth, and for repairing its constant deperditions, becomes, at the moment of birth, entrusted with more sublime functions. Assisted by the pulmonary organs, and receiving new principles from the atmospheric air, it imparts vigour and activity to the brain, which by its reaction influences and develops other powers from which proceed that irritability and sensibility distinguishing the animal from the other productions of nature.

This concurrence of powers appears simultaneous at the instant of birth, and on its partial or total derangement the life of the animal suffers or is annihilated. The cerebral organ soon becomes the principal agent, the chief *motor* of the whole machine. Without it no irritability, without irritability no sensibility in the

organs; all vital functions are at an end; the stimuli with which nature abounds become useless; the living fibre returns to the state of inorganic or dead matter.

From the vigour and agency of the medullary system depends the strength and abilities of the others; but this vigour would soon be consumed, if no new materials were furnished to replace the unavoidable deperditions. A mutual concord is therefore indispensable, and from it every organ receives the power of perpetuating it.

If the power of the medullary system upon all the organs is fully established by conclusive experiments, the influence of the stomach upon the brain is not less evidently demonstrated by daily experience; and a number of disordered affections, attributed to a redundancy of blood in the vessels of this organ are, on closer examination, found to be solely owing to an embarrassment of the gastric system. This is exposed to evidence by the symptoms succeeding the administration of an emetic, or by the presence of indigestible substances in the stomach: the vertigo experienced in these instances could not properly be referred to a determination of blood to the brain, when the pulse and the state of prostration of all the other organs attest a general and extreme debility. Indeed it is difficult, I may hazard to say impossible, to stimulate the stomach without affecting sensibly the head. I have so long embraced these opinions, and so often put them to the test, that I do not hesitate to say that I have cured more headaches by tonic than by depleting remedies.

But further, the facility and safety with which we can act upon the stomach, too generally known to require any comment, is extremely favourable; yet I am under the necessity of observing, that the former is much better known than the latter, for it is mercilessly abused without inquiring about its propriety.

My researches, however, were not directed at random, neither did I favour the course of the torrent, without previously ascertaining, with great caution, the effect of repeated stimuli upon the alimentary canal. Placing to proper advantage incidents and facts that are not unfrequently presented in the course of medical practice by the rash conduct of the ignorant, as well as

by the bold enterprises of the presumptuous, which so often demand the repairing hand and cares of the healing art, I soon discovered that the stomach accommodated itself much better to be tormented, and bore repeated depletion with less injury than the intestines; that the general strength was but little impaired after emetics, whereas it was considerably diminished, for several days, by cathartics; that the action of emetics was chiefly spent upon the stomach, and that of cathartics required the joint concern of the whole alimentary canal; from which complicated action different results were obtained.*

From these considerations, I could not hesitate to prefer emetics to any other medicines, for the purpose of producing upon the gastric organs, an action capable of being extended to the cerebral system, and enabling me to regulate and continue it as long as circumstances would require.

The effect contemplated could not be expected from a temporary stimulus producing only a transitory action; it required a more permanent agent, easy of management, and such as to be directed according to the constitution of the patient, and the obstinacy of the symptoms; for, in the long continuance of an energetic action upon the brain, did I place any confidence to obtain my purpose, fully convinced, as experience teaches us, that, by the repeated exercise of an organ, we increase its strength and vigour, which by habit, at last becomes permanent.

A saturated solution of tartrite of antimony made with equal quantity of distilled water and good Teneriffe wine, at a temperature of 55° of Fahrenheit's thermometer, was determined upon, on account of the facility of keeping it, and administering it in proper doses, on account of the increased proportion that would be necessitated by its repeated use.

It must not, however, be supposed, that I consider the strength of the solution of primary importance, for after the first administration, the subsequent doses may easily be regulated from the effect already obtained. The repeated use of this remedy is

* These remarks are still more applicable to children than to adults, although they may be considered as general.

perfectly safe, but it requires judgment and a careful hand not to overrate the dose: I have used it so often, and for so many years, without the least inconvenience, not only for children, but for adults of every age, that I am certain it may be relied on.

The first subject that I concluded to submit to this new mode of cure, was the son of Mrs. Garrick, then about one year old, and now between thirteen and fourteen. His head was about, and perhaps more than double of the natural size; the sutures had been extended to better than an inch in breadth; his eyes were rendered exceedingly small; and, which contributed to make the experiment doubtful, his body was of a rachitic conformation, and very much emaciated.

The father and mother entertaining but feeble hopes of the recovery of this child, having already tried in vain several remedies, did not hesitate to place him under my care, and cheerfully consented to follow my prescriptions.

We began with ten drops of the following compound in two teaspoonfuls of tepid water, and in forty minutes a smart vomiting came on, and returned again twice, shortly after:

R Solut. Satur. tart. Antim.	-	-	-	℥i.*
Tinct. Gentian.	-	-	-	℥i.
Syrup. Simp.	-	-	-	℥i.

The bowels were not moved by this medicine.

I directed the same dose for the next day, with injunctions to repeat one half of it, in case no vomiting should take place in the the course of thirty minutes.

The second dose, as I had supposed, became necessary; and favouring the vomiting with a few table-spoonfuls of lukewarm water in the intervals, they were repeated four times.

The following day, the same dose was required at three times half an hour intervals, to obtain four vomitings.

4th day. Fifteen drops at one dose: three vomitings.

For five ensuing days, the same quantity daily administered, effectuated two, three, and sometimes four vomitings.

This daily administration of the medicine was continued for

* This solution as described before.

one month, taking care to increase the dose gradually, until it arrived to twenty-five drops, which were always found afterwards adequate to the purpose. From the second week after the beginning of the treatment, the head began to decrease in size, and the countenance was much improved; the child took more nourishment, and his digestions were much better.

For the second month, the medicine was given only once in two days, and, the cure advancing rapidly, towards the end only twice a week.

In three months, I had the satisfaction to see this infant entirely well. The sutures of the head came into a closer contact, and ossification completed by degrees the solidity of the skull, which, after the removal of this disease, preserved, as it always does, not only a particular shape, but a larger appearance.

It was gratifying to my feelings to see this boy a few months ago, as hearty and smart a child as others of his age, and in no way deficient in his mental faculties.

Highly elevated by so complete a success, but more anxious to establish and support my discovery for the benefit of mankind, than to aim at the gratification of private feelings, and look for laurels to be buried in the dust of a book case, and not unfrequently, some years after, shaken by others, and appropriated to their own benefit, I carefully and silently pursued my inquiries, until repeated successes could crown my labours, and enable me to participate with certainty, their result to the world. I seized and looked for every opportunity to confirm the result of my practice, and obtained the same results in nine out of ten cases.

It was not, however, without extreme difficulty that I could prevail upon parents to repeat so often such an active medicine, and producing more alarming than hazardous symptoms. References were powerfully used, and procured the most effectual means; and on seeing the health of their children rather improving, I finally met with little or no opposition from them.

Were it not for the purpose of referring my readers to living records, I would not intrude upon their patience, nor impose upon the editor of this work, by introducing any other case; knowing too well, that it is too often resorted to, as the *adjuvans* to a too

VOL. III.—T t

small dose. But, as the subject that I now present is new, I hope my intentions will be more favourably interpreted, and I shall endeavour to be as concise and brief as possible.

At the beginning of the month of December, 1818, Mrs. — sister to Mr. S. Beaty, a baker, living in South street below Third, consulted me for her own child, whose head was enormously enlarged by the opening of all the sutures. It was about fifteen months old, and could not be left standing, without immediately falling upon the floor.

I was so bold of my former successes, that I went so far as to promise a cure; and pursuing the same mode that I have described, in the space of a month this child was restored to perfect health; and, with the help of the administration of tonics, in the course of another month longer, required no further assistance; it has continued to the present day, in an uninterrupted vigorous state of health, although it has, I am informed, suffered several severe falls upon the head, as is too frequently the case with all children.

The daughter of Mr. Vanarsdall, aged about eight years, exhibited symptoms of drowsiness and stupor, which increased rapidly for some time; the pupil became dilated, and she so often fell to the ground in attempting to walk, that her situation became alarming. Her pulse was low and slow, and her former activity had totally disappeared. Being the family physician, I was consulted, and did not hesitate to pronounce the cause of the existing symptoms.

By the daily use of emetics, in the course of a month she was restored to her former state of health; she has continued well since, although her activity is not as great, the continuance of the medicine having not in my opinion been long enough.

It is, I suppose, needless to produce other cases, as my intention is not to recommend and give authenticity to specifics and nostrums, but simply to expose my views and the mode of practice that I have persued, not only in this disease, but in other chronic affections of the head and other vital organs.

A number of subjects of different sexes, afflicted with habitual fits, and others with periodical headaches, have been either re-

lieved, or entirely cured by the repeated and long continued use of emetics. On the same principle, I feel disposed to believe, the sea-sickness has relieved or cured many disorders that had baffled the power of medicines.

Thus, from the reciprocal reaction of the brain, do we see, that the stomach is invigorated by the cheerful company of friends, by music and other pleasurable objects, and that a treble quantity of food may be received in, and digested by the stomach, at a festival entertainment, where all these stimuli act in concert upon our senses.

The most evident benefits have likewise been obtained from the same treatment, in cases of menorrhagia, from torpor and debility in the uterine system, as well as in various affections of the genital organs. It would be out of order to expatiate any longer upon topics unconnected with our present subject; I shall therefore conclude this communication, by expressing an opinion, perhaps not very generally entertained, that many diseases, and particularly those of a chronic nature, have oftener than it is supposed, their source in a want of vigour in the nervous system; and it will be found, and ascertained by experience, that all the remedies that exert their activity and produce an excitement upon the brain, the principal organ of that system, are the most beneficial to remove them.

Observations on the parts concerned in Lithotomy, which are intended to prove that Mr. Pattison's ideas of a Prostate Fascia are erroneous. By Horatio G. Jameson, M. D. of Baltimore.

HAVING engaged in a course of dissections, with a view of reviving my recollection of anatomy generally, my attention was particularly called to the structure of the bladder and its connections, which are concerned in the operation for the stone, in consequence of hearing a good deal said, by different medical gentle-

men, about a paper written by Mr. Pattison of Philadelphia. I understood this gentleman had made an important discovery, which was likely to lead to a new operation for the stone.

As I was then dissecting the muscles, I went on in the usual way to dissect those of the pelvis, believing that if such a thing as a *prostate fascia* existed, I could not well miss finding it; but, in this I was much mistaken, for after the most careful dissection I could find nothing new.

Being assured about this time, that Dr. Physick had acknowledged the existence of the *prostate fascia*, I resolved, anew, to repeat my efforts to discover it; and in order that I might be enabled to do the thing in the right way, I procured the paper giving Mr. Pattison's ideas of the subject. I found, on perusal, that this *fascia* could only be found by a particular method of managing the dissection of the parts.

I procured a new subject, and attentively followed the direction of our author. Having divided the symphysis pubis, and made the separation of the bones recommended, I was greatly surprised, and greatly gratified to find such a piece of structure as has been described in the paper alluded to.

For the truth of what I say, respecting the pleasure I experienced on finding this *fascia*, I can refer to my worthy young friend Dr. Dorsey of this city. And further, I can most truly declare, no one is more willing than myself, to adopt whatever improvements may appear in practice, which bear the test of careful trial, perfectly regardless of their source.

Here then, it will appear, I was a complete proselyte to this anatomical discovery; but I was still unsatisfied as to the deductions our author had made. This, together with a wish to examine critically into the operation of lithotomy, more particularly so far as the gorget is concerned, induced me to make a new set of experiments.

After different experiments had been made, by operation, and by dissection, I made the following preparation: (it is preserved, and will serve to establish the accuracy of my opinion on the question of anatomy connected with the case under considera-

tion). A lean subject was obtained, the thighs amputated, the lumbar vertebræ divided; and the pelvis thus separated from the body. The bladder, which was empty, and contracted close up under the pubis, as also the urethra, were injected with the common wax injection. That portion of the peritoneum which lines the pelvis, and is attached to the lower part of the bladder, was separated from the left side of the pelvis, which was easily done, without cutting one fibre. The obturator muscles were now seen beautifully spread in a funnel-like form, around the lateral and anterior surface of the bony structure.

Now dissecting around pretty low down, on the inner surface of the obturator muscles, a membrane was easily raised all around: pursuing this a little way with the knife, till I had cut the muscles just named carefully through, I came into contact with the fibres of the levator ani muscle. The symphysis pubis, the sacrum and the coccyx were sawed through longitudinally, and the bony pelvis thus separated. The remaining soft parts were divided carefully on the left side, so as to leave the bladder untouched.

The pubis having been thus divided, without cutting away any part of it, it is evident, I had every thing which belongs to the parts under consideration. And lifting up that part of the obturator muscle, which I had cut off, I had a distinct view of a membrane which suspends the bladder, having a convex form; and which, passing off from the obturator muscles, and from the os pubis, is connected with the bladder at its neck. Innis says of this membrane, speaking of the levator ani muscle, after mentioning its different origins, that it arises "from the thin tendinous membrane that covers the obturator internus and coccygeus muscles." And, examining this membrane, I find it spread over nearly all the anterior and lateral parts of the pelvis to which the levator ani muscle is connected: consequently it passes off from the obturator muscle, and under the os pubis; embraces the neck of the bladder somewhat behind the prostate gland; passes down over the vesiculæ seminales, and is attached to the rectum and to the coccygeal muscle.

Now, from this description it will appear, that this membrane is the only one connected with the neck of the bladder; and that it is

spread inside of the pelvis, around its lower brim, and a considerable distance up, and within the os pubis; and, therefore, it goes a great way above the prostate gland. And, moreover, it is an assemblage of membranous bands, which have been viewed by some as distinct membranous ligaments. I shall, for the sake of illustration, call these by one name; and, that of *cystick membrane* seems appropriate.

From what has been said, it will appear reasonable, that if we divide the symphysis pubis, and make a separation of an inch and a half, we will bring down some part of the membrane which I have just described; and bring it in folds across the prostate gland in a stretched and tightened condition, and thus it acquires that imposing form attributed to the *prostate fascia*.

All this can be most clearly shown by a single dissection of a lean subject; and, for the sake of illustration, I will briefly recapitulate what I have said, and prove more incontestably, that the *prostate fascia* is but an accidental state of the parts, and the effect of violence.

If I were disposed to ascertain merely a question of anatomy unconnected with surgery, I would most willingly rest the subject upon what has been said; but as I am decidedly of opinion, that the deductions and practical instructions, which are derived hence, are erroneous, I think proper to say something respecting this part of the subject also; believing, that if our premises are erroneous, our deductions cannot well be otherwise.

RECAPITULATION.

To obtain a view of the bladder perfectly correct, as relates to ligaments, &c., the following method may be pursued. From a lean subject, take out the entire pelvis—strip the peritonæum from the left side of the pelvis, from the middle of the sacrum to the symphysis pubis—cut down the obturator muscles from the bone, and leave them attached to the levator ani muscle—divide the skin along the symphysis pubis down to the penis, then turn a little to the left of the penis, or (as the subject is supposed to lie with the coccyx downwards, and the dissector looking up-

wards,) you carry the knife to the right of the raphe of the perinæum; now get to the middle of the coccyx and sacrum, and thus you have divided the skin longitudinally through its whole length. Dissect up the crus penis, on the left side of the subject, and cut it off—take up the sphincter ani on the same side—divide the erector penis, and transversalis perinei muscles—saw the symphysis pubis, and through the middle of the coccyx and sacrum.—The division is now made, except a part of the levator ani and coccygeal muscles; cut these as low down as you can, and carefully dissect the levator ani from the bone of the pubis.

Having now divided the pelvis without taking away one fibre, it is evident, that if there is a fascia passing from the “obturator internus,” it is now to be seen. But, the fact is, that if you take hold of that part of the obturator which I have advised to be cut off, and left attached to this membrane, you may spread this membrane so attached, up over the bladder, to its natural position; and you then find it to be a membrane of an irregular structure, and about equally strong throughout its extent, but seems to be made up of bands or several pieces. It extends, perhaps, an inch and a half above the prostate gland, being rather behind, or more inward than the gland, that is, along the outer edge of the gland. From this membrane the levator ani muscle has much of its origin; it is closely connected to the neck of the bladder, goes down over the vesiculæ seminales, and is tied down to the rectum and coccygeal muscle.

By putting this *cystick membrane* gently upon the stretch, by holding up that part of the obturator muscle, which I leave attached to it, you most plainly see that by separating the ossa pubis “an inch and a half,” you will bring down a part of the membrane, which is, naturally, above the neck of the bladder, across the prostate gland; and getting into folds, it gets the imposing appearance of a ligament. I shall conclude with a few remarks upon Mr. Pattison’s practical inferences:

We are advised to open the membranous part of the urethra—cut five lines through the left lobe of the prostate gland, and then, in like manner, on the right, and making both incisions obliquely lateral, we form an opening, having the form of the letter lambda,

A. Now, I find, that when the *cystick membrane* is in its proper situation, that, if we “lateralize” a little too much, we cut in the worst possible direction, for this membrane passing straight off from the edge of the prostate, to be carried over, and spread upon the obturator internus muscle, leaves a very small space between the gland and the cavity of the pelvis, in which the bladder lies. But this membrane being carried downwards, along the *vesiculæ seminales* to the rectum, makes a downward direction most safe; and by cutting nearly downwards, along the left edge of the prostate gland, we can go fully to the extent of the cutting edge of the common gorget, without going inside of the attachments of the *cystick membrane*, that is, without cutting into the cavity of the pelvis beyond this membrane.

It is only by putting the parts into a state of derangement, as every man must admit they are, when we make a separation of an inch and a half, that this lateral incision is indicated. And I think I have undeniably demonstrated, even admitting the existence of a membrane or ligament which has not been known before, that Mr. Pattison’s ideas are erroneous; for by “lateralizing” a little too much, we will, of all other directions, by this one, come soonest into the internal cavity, which we are admonished to avoid.

Can any thing be more plain, than that if we force the bones of the pubis an inch and a half, in a lateral direction, that we thereby, spread the attachments on the sides of the bladder, to an unnatural extent, and thus we may be led into error; for, laterally, the distance which we may safely cut is very small; but downwards it is considerable, when the parts are in their natural state; but this state of things is reversed somewhat by forcibly separating the *ossa pubis*.

In short, I have demonstrated to my most entire satisfaction, that no such thing as a *prostate fascia* exists, according to Mr. Pattison’s idea of it. And if his premises are *erroneous*, his practical inferences cannot well be less *erroneous*; careful experiment will prove the whole absolutely *erroneous*.

I believe Mr. Pattison’s cutting instrument to be less dangerous than the common gorget, because it is shorter in the blade; but

both, in my opinion, are greatly inferior to a common knife. I have gone through a set of experiments which clearly prove, that we can easily and safely cut into the bladder without using a gorget or a grooved staff. But as I intend to prosecute these experiments still farther, I shall avoid for the present, saying any thing on the operation which I intend to adopt, and of which I purpose to give an account, so soon as I have more satisfactorily matured my experiments.

The idea of dilating the neck of the bladder to any considerable degree, as recommended by Mr. Pattison, in the paper before us, is to my apprehension truly frightful. I would much rather agree with the advice given by John Bell, that where the stone is very large, it ought to be broken, by means of very strong forceps, while in the bladder. The whole muscular structure of the neck of the bladder, performing ordinarily the office of a sphincter muscle, would render such an attempt superlatively dangerous.

Observations on Certain Articles of the American Materia Medica. By Stephen Burgon, M. D. of Bucks County, (Pa.)

(Communicated by James Mease, M. D.)

1. *PODOPHYLLUM PELTATUM*, or May apple.—This is a common plant in this county, and I believe in most counties of the state; as well as many of those of Ohio. The powdered root is extensively employed as a cathartic in bilious complaints; and I am persuaded with as much success as the jalap. I have often prescribed it combined with calomel in the proportion of 20 grs. of the former, to 8 or 10 of the latter; and have uniformly been pleased with its effects on my patients. In this dose it is extremely prompt and efficacious. My experience enables me to state, that it is more drastic than jalap, and of course occasions more active catharsis, more severe griping, and makes a more permanent impression on the system. Its operation in all

VOL. III.—U u

cases in which I have administered it, is slower than that of jalap, but it leaves the bowels longer in a lax and soluble condition. I once took 20 grs. at 4 o'clock P. M. which gave me no disturbance till the next morning, when its operation commenced and produced continual motions all that day, and part of the next night, together with severe tormina; this was the first dose of podophyllum I had ever administered; and its effects being so decided, I have since prescribed it in a multitude of cases, and for the most part, with similar results. Like most other drastic cathartics, it is rendered milder by combining it with calomel, and hence in most cases this combination is to be preferred to giving it alone. It is more disagreeable to the stomach than common purgatives, and will oftener occasion emesis; in bilious affections it usually supersedes the necessity of an emetic previous to a cathartic, and hence two desirable effects are produced by one agent.

In all fevers where active purging is indicated it answers extremely well: united with crem. tart. I have prescribed it in anasarca and ascites, with evident benefit to my patient. I have not yet had an opportunity to try it in neuroses; but I am persuaded it will be found singularly useful in this family of untoward and provoking diseases. The first cases of chorea, epilepsy, &c. which it may be my lot to attend, shall be treated by it.

The podophyllum is one of those articles which operates alike on man and horse. This I ascertained in a tour I made to the west in the spring of 1814. Having foundered my horse by giving him a mess of Indian corn; after he had been on short allowance for two or three days, I was led to theorize upon that affection; and though I had never paid much attention to horses, and none to their diseases, it occurred to me that it must be a disease of the stomach and alimentary canal, and that if he could be actively purged the disease would pass off. Being twelve miles distant from any place where medicine could be procured, I was necessitated to try a decoction of podophyllum; which I observed growing plentifully around the house. I accordingly procured a considerable quantity of the root, had a strong decoc-

tion prepared, and drenched him completely therewith, without any regard to quantity. I presume we poured down him two or three gallons of a saturated decoction, and left him for the evening; before morning it had purged him actively, and I was enabled to proceed on my journey, to my no small satisfaction.

The May apple is more employed in regular practice than any other indigenous cathartic; and deserves a place in all our shops.

2. *Eupatorium Perfoliatum*.—This plant is in plenty in this vicinity; my experience with it is limited to cases of anorexia, consequent to drunkenness. In such cases I have used a cold infusion with evident benefit, and I prefer it to any article I have hitherto employed; it very speedily restores the tone of the stomach, and no unpleasant effects follow its administration.

3. *Chimaphila Maculata*.—A gentleman of veracity lately told me of an obstinate case of tinea capitis (which had resisted a variety of the ordinary remedies) speedily yielding to an ungt. made of the leaves of this vegetable.

4. *Zanthoxylum Fraxinifolium*, or prickly ash,—grows in many parts of this county, though not in this immediate vicinity. I procured a quantity of it from my friend John Hahn, M. D. of Montgomery county; but as yet have made no use of it. The capsule of the berry is extremely acrid, and I have no doubt contains an essential oil. A tincture of the bark is a domestic remedy for chronic rheumatism. A lady in Buckingham who had taken a considerable quantity of the tincture for rheumatism was completely salivated by it, and challenged her physician with having given her mercury. I have heard much of its salivating, but have never witnessed this effect from its administration: chewing the capsules powerfully promotes the flow of saliva.

5. *Prunus Virginiana*.—My experience with it is limited to a few chronic cases of asthma, and similar affections. In these diseases it is an important auxiliary and worthy of confidence; it should never however be given where the lancet is indicated. My friend Emmor Kimber (of Chester county) informed me of a horse-jockey in the state of New York who was in the practice of purchasing horses with the heaves, for small sums; and by restricting them to a wet regimen and drenching them freely and fre-

quently with a decoction of this tree, so improved their condition as to pass them for sound horses; whereby a handsome perquisite was clandestinely obtained to the sorrow of many an honest yeoman.

6. *Asclepias Tuberosa*.—This is deservedly a popular remedy in many diseases, and worthy the attention of every American physician. I shall speak of its application to three diseases in particular, viz. dentition, cholera infantum, and marasmus.

1st. In *Dentition*. Robust and vigorous children are predisposed during dentition to inflammation, diarrhœa and fever; the latter frequently terminating in convulsion, speedily followed by death, while those children of a contrary habit of body, are generally exempt from these unpleasant symptoms, and for the most part pass through the progress of dentition with comparative facility. Where plethora therefore attends, venesection and cathartics are obviously indicated. It is of great importance to attend to the latter indication; it being a fact of notoriety that those children whose bowels remain open and regular, pass through the process of dentition with more safety and ease, than those whose bowels are either in a costive or irregular state. In both of these conditions purgatives are equally proper, and perhaps none answers better than the *asclepias tuberosa*, given in doses of from 6 to 8 grs. twice in twenty-four hours. This medicine is peculiarly adapted to the diseases of children which require purging; as they freely take it, from its not possessing either a disagreeable taste or smell. It operates mildly, never produces griping, and always leaves the bowels in a composed condition. This article is also a most active diaphoretic. To produce this effect the following formula, which I obtained from my friend Samuel Shuman, M. D. of Hagerstown in Maryland, appears to answer perfectly well:

R. Rad. Asclep.	-	-	-	℥ii
Lav. Recent.	-	-	-	℥xviii

boiled down to ℥xii. One ounce of this decoction may be given twice or thrice in twenty-four hours; it excites a copious perspiration, and proves at the same time gently cathartic, and is therefore capable of extensive application in the practice of medicine.

2d. *Cholera infantum*. In the treatment of this terrific disease, after freely evacuating the alimentary canal, by a dose of cal. et jalap, it becomes important to keep them in a soluble condition; this may be effectually done by the asclepias in small and repeated doses, conjoined with some aromatic substance, such as cinnamon or lavender. When given in this manner it purges gently, and never leaves the bowels disposed to costiveness. Here also the decoction above mentioned is highly beneficial; by not only purging, but likewise producing diaphoresis, which, in diseases of the bowels, is an operation always followed with benefit. This is now, I am credibly informed, a common practice in many parts of the southern states, and to its efficacy I can bear ample testimony.

3d. *Marasmus* or atrophy, has, since the publication of Hamilton on Purgatives, been frequently treated by "active purging long continued," and I am happy in having it in my power to add my testimony to the correctness of the practice. The patient when under this treatment becomes (as Dr. Chapman observes) strong and corpulent, and his general health improves daily.

7. *Arum Triphyllum*.—The root is the only part of this plant hitherto used in medicine, though every part of it abounds with an extremely active juice; the berry contains it in the greatest degree, nearly as two to one to the root, it also is more retentive of its peculiar acrimony than any other part of the plant, and if examined in the depth of winter when the ground is covered with snow, it is found extremely pungent and unpleasant. The root is more acrimonious than either the leaves or stalk. The pungency of the recent plant is speedily obviated by drinking new milk, as I have repeatedly witnessed. The largest roots should be chosen for medicinal purposes; they should be gathered when the plant has ripened its berries and begun to decay; as the root is then least succulent and shrinks least in drying. In this state, if buried in moist sand, they will retain their active properties for months, and even years, with very little deterioration, provided they be kept moist only.

The arum has long been celebrated for its efficacy in certain diseases, and I am persuaded not without reason. I have never prescribed it where the lancet was indicated, as I believe such a practice would be highly prejudicial; but in chronic cases of asth-

mas, catarrhs, &c. I place unlimited confidence in its medicinal virtues. In catarrhs of long standing, and in debility and oppression of the stomach, the arum is deservedly a popular remedy in domestic practice, and of late its application has been sanctioned by the authority of many eminent physicians. I know from repeated and various trials, that in such cases its virtues have not been overrated, and believe it entitled to all the attention that can possibly be bestowed upon it. A certain quack near Easton in this state, acquired considerable eminence in the treatment of such diseases, by the use of the arum: he affected numerous cures that had resisted ordinary measures prescribed by the faculty.

In asthma, consumption and croup, my experience with it is limited; but I believe it highly useful, and wish it more generally resorted to. I would not employ it in croup till after copious depletion, when cough, hoarseness and tightness of the chest and difficult expectoration remain; I believe it is in extinguishing the remains of croup, that it has been found so singularly useful. I once prescribed arum in the advanced stage of a cure of genuine phthisis pul. which had resisted the whole routine of regular practice; but with no permanent advantage; it very remarkably palliated the urgency of the symptoms, particularly the difficulty of breathing and cough; the patient, however, ultimately fell a victim to the disease. From what I know of the *A. triphyllum*, and what I have read of the *A. maculatum*, I am persuaded they are nearly allied in medical properties, and applicable to the same forms of diseases.

The root of *A. triphyllum* may be exhibited in milk or covered by unctuous and gummy materials, in form of electuary or emulsion; but the formula I prefer and have most employed is that of conserve, made agreeable to the formula of the conserve aur. of the London Dispensatory; and in similar doses.

P. S. In August last, I was employed one afternoon in a close room, in powdering the rad. podophyllum; which by the next morning occasioned a most violent inflammation of my right eye, and eye-lid; it however yielded to the antiphlogistic regimen in eight or ten days. I lost 50 or 60 ounces of blood, took physic, &c. was blistered, &c. &c.

On the Adulteration of Malt Liquors in England.

IN the third volume of the Medical Museum, of Phila. for 1807, and the first volume of the Medical Recorder, may be found two papers by Dr. Mease on this interesting subject; and the conclusion he drew from the facts he detailed, was, that medical men ought to confine their patients to the wholesome and substantial liquor brewed in the United States, and to interdict the use altogether of foreign malt liquors. A late publication by Mr. Accum, an eminent operative chemist and lecturer in London, entitled "*A Treatise on Adulterations of Food, and Culinary Poisons,*" amply confirms the statements of Dr. Mease, and gives, besides, a shocking detail of the sophistications practised in London, in numerous articles that daily enter into the diet of the citizens. With respect to malt liquors, it appears that the practice of adulterating beer by *coccus indicus*, a highly intoxicating drug, was of early date, and severe penalties were imposed upon persons using that article, by an act passed in the reign of Queen Anne. In later times, the use of that and other noxious ingredients, has increased to a most astonishing extent, and in consequence of a strict examination of the subject, and of many persons by a committee of the House of Commons, specially appointed for the purpose, the whole of the adulterations, and noxious ingredients employed, have been ascertained. The law prohibits the brewers from using any ingredients in their brewing, except malt and hops; but as the excise upon those articles is great, and constitutes a principal item in British revenue, various articles are substituted for the purpose of giving the taste, body, briskness, colour, and intoxicating quality (an important point) of the genuine article, in order to avoid the excise duties, and, of course, to increase their profits. Considerations of health do not enter into their calculations. To such a height had the practice risen, that a set of men followed the particular business of preparing and selling the noxious and illegal articles; they were called *brewers' druggists*, and issued their prices current to the trade regularly. Besides the foreign

articles mentioned by Dr. Mease as in common use, Mr. Accum adds sulphuric acid, alum and salt; these are added to give new beer an old flavour: fortunately, they are used only in small quantities, and are not injurious. The practice of selling spoiled beer, mixed with the leavings of pots, the bottom of butts, drippings, and remnants that lay in the leaden pipes of the brewery, after mixing them with a portion of the fresh or brisk article, is carried on to a great extent. Mr. Accum confines these scandalous practices to country brewers and retailers: the eleven large breweries of London, are wholly free from them. Lists are given,

1st. Of 29 druggists and grocers convicted from 1812 to 1819, for supplying illegal ingredients for adulterating beer.

2d. Of 19 publicans convicted for adulterating beer with illegal ingredients, and for mixing table beer with strong beer.

3d. Of 19 brewers convicted for adulterating strong beer and table beer.

4th. Of 34 brewers convicted for receiving and using illegal ingredients in their breweries.

5th. Of 9 persons in whose possession a vast variety and quantity of illegal ingredients were found, which are specified.

All the foregoing persons were fined in different sums from 20*l.* to 500*l.*

From Mr. Accum's book, it appears, that deleterious, and actually poisonous adulterations, to an astonishing extent, prevail in wine, brandy, gin, home made wines, bread, flour, cheese, Cayenne pepper, pickles, *confectionary* catsup, custards, anchovy sauce, olive oil, mushrooms, and that universal article *tea*; and that wine, brandy, bread, coffee, pepper, vinegar, cream, lozenges of ginger tolu or magnesia; mustard, and lemon acid, all undergo various sophistications and mixtures, which tend to diminish their wholesome qualities, or to defeat the intent of their use. Coffee and pepper are counterfeited by substituting beans and peas; and tea, by using the leaves of the sloe, ash, and elder, and other leaves, coloured by logwood, Dutch pink, verdigris and copperas. The whole development affords a melancholy picture of the moral state of society in England.

Case of Recovery from Drowning.

IN the evening of the 22d of November, 1803, a man of about 40 years of age, when in a state of drunkenness, had a rencontre with several sea-faring men, who pushed him into the river, at Race street dock. Some ship-carpenters and shallopmen, who were carousing in the cabin of an adjacent vessel, heard the affray, but did not immediately enquire into the cause of it, but on returning from on board their vessel near an hour after, they accidentally discovered the body of a man floating between a vessel and the wharf. They directly drew him out of the water, and conveyed him upon a wheelbarrow to a neighbouring sailor tavern. At 10 minutes past 11 o'clock P. M. the late Doctor Cathrall was called to visit him, who found him lying on the floor with his wet clothes on. His face was pale, his mouth and eyes were wide open, and the whole body was cold, and appeared destitute of life. The wet clothing was directly stripped off and the body dried between blankets, and as soon as some warmed salt could be procured, friction was applied with it to the whole surface of the body, and heated bricks were constantly kept to the bottoms of the feet. This practice was diligently persisted in until a quarter past 12 A. M., but there was not the least appearance of returning life. The people considered the case as hopeless, and refused to make any further exertions.

The room, which was otherwise uncomfortable, had become damp and cold, on account of the people going out into the rain and returning into the room with their wet clothes, and several of the men being in a state of inebriety and turbulence, the Doctor also became discouraged, and went to the door intending to go home, but finding the rain falling in torrents, he returned to the room and persuaded the people to make a renewed effort to restore the unfortunate man. This the Doctor did more to employ the men till the rain should abate, than with any hope of success. The frictions were vigorously renewed, and the lungs were inflated by means of the apparatus belonging to the Humane Society.

At half past 12 A. M. the body felt rather warmer, and there was a faint tint of redness diffused over the face and forehead, accompanied with a strange noise in the wind pipe, which appeared to be a feeble attempt to commence respiration. They now resumed their labours with more alacrity, and procured as free a current of air as the confined situation would admit, and in the space of ten minutes a very gentle and general warmth was spread over the body. He now attempted a deep and more full respiration, which was attended with great labour and anxiety, and was not repeated for a considerable interval, in which respect it seemed to resemble the respiration of a person in the act of expiring. Some slight pulsation could now be perceived in the region of the heart, and also in the large arteries of the neck. All these appearances of recovery gradually increased as the friction was continued, and the respiration became more frequent, but still difficult and apoplectic. The pulse gained strength and frequency at the wrist, but on a sudden the face became more livid, and he appeared as if going into a fit. Suitable medicines were administered, and gentle frictions continued, and in the course of half an hour, he was so far recovered as to be able to tell his name and occupation, and the next morning he returned to his home.

The foregoing case was published in a newspaper in Philadelphia, Sept. 16, 1806. But I think it worthy of being brought in a more respectable manner before the medical public, chiefly with a view of shewing to them, the great importance of the efforts of reanimation being longer continued than they generally are. I have long entertained the opinion that the want of success which commonly attends the attempts at restoring drowned persons, is owing to the cessation of the use of the means after a trial of them for half an hour or little more, from an opinion that the person is irrecoverably dead. Of the facts as related above, there can be no doubt, and if all the circumstances are duly considered, they will afford as strong grounds for perseverance as any on record.

J. M.

On the best method of removing Contractions in Limbs from Burns.
By William G. Nice, M. D. of Manchester, Virginia.

DEFORMITY arising from burns in various parts of the body, though a very common occurrence, is one that, I am persuaded, has not excited that attention from physicians, which its importance merits. My principal object in presenting this paper to public notice is, to make known a case of contraction and consequent deformity of a limb from a severe burn, which was effectually relieved by the method of operating and subsequent treatment, as detailed below.

The principle upon which the operation in this case was founded, is by no means original; but was suggested by the perusal of a valuable paper upon the same subject, by Henry Earle of London, as related in one of the volumes of the *Medico-Chirurgical Journal*. That intelligent author having noticed several cases of deformity from burns, in which a simple division of the contracted parts, repeated even for several times, proved of no service whatever, formed the idea of relieving patients labouring under contractions of the limbs from burns, by removing the whole of the old cicatrix; he accordingly describes an operation which he performed on the arm of a boy, which was attended with perfect success.

Previous to relating the following case, I shall beg leave to remark, that I believe every operation, consisting of a mere division of the old cicatrix, for the relief of limbs contracted from the effects of fire, will prove generally unsuccessful. I have had an opportunity of witnessing several operations, all of which went to establish the truth of the above position. Thus, I have known a division of the superior eye-lid, which had become contracted from a burn, though repeated several times, to prove of no advantage whatever; I have known a simple division of the integuments of the neck, where the chin had been drawn down, prove equally unsuccessful; I have also heard of an incision through the contracted integuments of the ham, though repeated several times, which proved of not the least service. The method of operating in these cases recommended by Dr. Dzondi, and related in the *Me-*

dical Recorder, it is true, possesses some advantages over a simple incision, but the objections against it are, I think, sufficient to prevent its ever being attempted; for if, as he tells us, an incision was repeated no less than three times, to relieve a contraction in the small muscles of the face, what would have been the effect, if the same had been repeated as often, to relieve contraction existing in the large muscles surrounding the knee joint.

Mr. Earle has very justly remarked, that these contractions are often improperly attributed to the carelessness of the nurse or surgeon, in not keeping the limb properly extended; but, that this is not the fact; for the disposition to contraction after burns is such, that all the care and attention which may be bestowed, will often prove ineffectual in preventing deformity. Why this disposition to contraction after burns exists, in a greater degree than in ulcers from other causes, cannot, perhaps, be satisfactorily explained; it is sufficient for our purpose, that this disposition to contraction does exist. An ulcer formed in the old cicatrix of a burn, even for years after the primary disease has been cured, will still retain the same features which characterized the original affection; it is for this reason, therefore, that a simple incision through an old cicatrix, will so seldom succeed in removing contraction; for, being situated in the parts formerly ulcerated, it only produces a repetition of that disease, the effects of which, an operation is intended to remove. Mr. Earle having observed the above fact, formed the idea of operating in such cases, so as to remove the whole of the old cicatrix, with the view of producing in its stead, a simple ulcer, existing in sound parts, which could be healed by the usual treatment in such cases, unattended by the phenomena, so troublesome in ulcers, after burns. The case detailed below is, I think, sufficient to prove the possibility of removing the most dreadful cases of deformity, from contractions after burns.

In January last, I was requested to examine Miss C—— W——, aged seven years and seven months, who, I was informed, was very lame, in consequence of a severe burn received when about a month old. On seeing her, I found the left leg so much contracted, as to prevent her touching the floor with her

toes, when standing erect on the other leg; the foot of the affected limb shorter than that of the other, and somewhat turned out. From the upper back part of the thigh, there was a mass of thickened skin and condensed cellular substance, commencing at the lower part of the nates, running down the back of the leg, and terminating nearly as low as the tendo achilles. Immediately in the ham, this substance was more than an inch in thickness, and from frequent attempts to straiten the leg, it was, in this place, in a continual state of ulceration. The inner hamstring felt perfectly natural, though evidently contracted from the permanent flexion of the joint; the tendon of the biceps flexor, or outer hamstring, could, with difficulty, be felt lying deep in the fold of the joint, at the base of the thickened mass above mentioned, which lay somewhat on the outside of the outer hamstring, so as to form a strong dense ligament, of rather a triangular form, which kept the knee joint permanently flexed, at an angle of about 100 degrees.

I advised an immediate extirpation of the whole diseased part, for which purpose she was removed to this place; and on the 7th of February, assisted by Drs. Pattison, Nelson and Tabb, I proceeded to operate in the following manner. The patient being laid on a table on her face, and well secured, by assistants, I made an incision on the inner side of the mass before mentioned, carried it along the base of it, to within a short distance of the buttock; then down, on the outside of it, in the same manner, until it reached the knee joint, exactly opposite the commencement of the first incision; owing to the great thickness of the contracted parts and the hardness of the old cicatrix, several incisions were necessary, to dissect the whole of the superior portion of the superfluous substance from the thigh; as soon, however, as this object was effected, the leg immediately straitened considerably, and the whole of the organized mass fell down upon the calf of the leg; from which I dissected it off immediately, so as to leave an extensive wound, of 13 inches in length and between three and four broad; I now very cautiously divided the cellular and adipose substance, in the bend of the ham, until I exposed the tendon of the biceps flexor cruris and

the tendinous membranes, which surround the popliteal vessels; at each stroke of the knife, the leg became evidently straighter, as the contracted fibres were divided. The incisions on the inner side, were entirely in the sound parts, those on the outer side, were necessarily in the old cicatrix, from the great extent of the burn. Although the leg had become much straighter, yet, it was still somewhat bent, evidently from a contraction of the flexor muscles of the thigh, from long continuance of the joint in a flexed position; the knife had removed every mechanical impediment, and I was in hopes, permanent extension would succeed, in straightening the leg entirely. The patient was now put to bed, and a carved splint, to fit the front part of the leg, though somewhat straighter, was now applied, and a bandage brought down over it, so as to keep the leg as much extended as possible; the patient suffered very much during the night, from the tension produced by the splint and bandage, which caused frequent spasms of the affected limb.

Feb. 8th. On examining the leg this morning, I found it had become evidently straighter, and another splint of a less angle was now applied, and the leg drawn as close to it, by the bandage, as prudence would permit. Fever not very high.

11th. Although the leg continued to become straighter, yet, to our great mortification, we found that the knee had become very much inflamed, from the pressure of the splint upon it; every thing was now resorted to, which was calculated to remedy this; the splints were stuffed and lined with buckskin, holes were cut in them, so as to remove pressure from the affected parts; but nothing appeared to answer sufficiently well, so as to keep up extension, without increasing inflammation; the knee was now considerably ulcerated, and it was evident that this plan of extension, could be persevered in no longer. Dr. Physick's apparatus for fractured thighs, was now proposed by Dr. Pattison and applied, which seemed to answer extremely well, in keeping up a permanent extension, without producing much irritation about the knee joint. The ulcer being considerably inflamed, a poultice was applied.

18th. On measuring both limbs this day, no difference what-

ever can be observed in their lengths; nevertheless, it was thought prudent to keep up extension by the splints, for fear of a recurrence of contraction; the muscles having acquired a wrong action from long flexion of the leg. The ulcer is at this time treated by adhesive strips, drawn across the ulcer, so as to produce lateral contraction during the healing process.

30th. The ulcer is at this time healing slowly; immediately in the ham, it has a great disposition to fungous granulations, which would in the course of a night increase so fast, as almost to reach across the inner part of the ham; and seemed to be an effort in nature, to reproduce a substance, similar to that which had been removed; the removal of this, and its future increase was prevented by laying a strip of adhesive plaster immediately in the joint and drawing it tightly across; over this, a bandage was applied so as to make considerable pressure.

Although the ulcer has diminished considerably in size, from the treatment mentioned above; yet it seems to assume an indolent aspect, and frequently breaks out in one part, while healing in another; astringent lotions were now ordered, with a view to strengthen the small vessels.

April 5th. The ulcer has now healed up on the calf entirely; though of considerable size, on the thigh. Upon placing the patient in an erect posture, the gravity of the blood was so great, as to overcome the thin coats of the small vessels, and ulceration was again produced on the calf; we now determined not to suffer the patient to assume an erect posture, until the leg should become entirely healed.

May 7th. The patient is at this time nearly recovered, and can walk a few steps with assistance; her general health having suffered somewhat, she has been directed to take bark; on the 10th she was sent home, with strict injunctions to persevere in walking every day, and to apply the extending apparatus every night, and to keep a bandage constantly applied.

June 10th. I visited the patient, and was agreeably surprised to find that she had perfectly recovered, and was able to walk with perfect ease; although small ulcers occasionally appear, yet

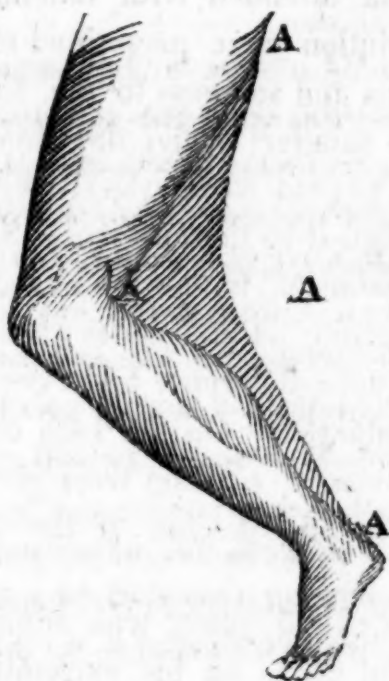
they are attended with but little inconvenience; she has perfectly recovered her health, and from a poor decrepid little cripple, she has become a fine blooming girl, with scarce a vestige of her former misfortune. A bandage is still kept around the leg and the splints applied at night; though there is not the slightest difference in the length of her limbs.

There are two circumstances to be noticed here, upon which the ultimate success of this operation will, I think, in a great measure, depend. First, in preventing excoriation; this is done in the perinæum, in the usual manner, by a soft pad on the top of the splint; but, at the ankle, it can only be done effectually, by making a padded cloth gaiter, to fit neatly over the instep and heel, lacing on the outside; this is to be placed over a soft handkerchief, which is first to be carefully wrapped around the lower part of the leg and ankle; a loop is sewed across the bottom of the gaiter, through this a handkerchief is passed and tied to the cross piece at the bottom of the splints; extension is by this means graduated at pleasure; an apparatus of this kind, was the only thing which answered the double effect of keeping up the extension and preventing excoriation, at the heel and instep. In the second place, it is essential, that a strip of adhesive plaster should be folded and applied tightly across the ham, as there is a great disposition to luxuriant granulations in that place, which would adhere in their growth and reproduce contraction; the pressure from the strip, in the above instance, was sufficient to keep these down; indeed, I am confident, that without a constant application of the adhesive plaster, in the manner above mentioned, permanent extension alone would not have proved sufficient, in preventing the reproduction of contraction, from the adhesions of luxuriant granulations; as in this way the disease seems to have been originally produced.

The dressings during the case, were of the most simple kind; consisting of the mildest poultices during inflammation, and adhesive strips, whenever they could be applied. Epsom salts were frequently given to open the bowels, and great benefit was obtained from the administration of bark, in the latter part of the cure.

Should the above detail be the means of procuring relief, in any similar instance, the purpose for which this was written will be fully answered.

Annexed is a rough plan of the leg previous to operating.



A A A A, the substance which produced the contraction, and which was entirely removed by the knife.

On the Use of Fowler's Solution in Nodes. By Samuel Colhoun, M. D.

NODES are obstinate, sometimes terminate in caries, are generally painful, and often incurable. They have been removed by the arsenical solution of Fowler, in the following cases.

This remedy has been known to me since the year 1815. On the 5th of January, 1817, J. Richardson consulted me with nodes on the ulna and tibia, near the knee. They were very painful, and of the size of a walnut. Ten drops were prescribed thrice a

VOL. III.—Y y

day. In two weeks, the nodes had abated in size, the pain had lessened, and in about three weeks he was well.

The following cases occurred in the Pennsylvania Hospital:

John Duncan, a sailor, aged 22 years, had large nodes on his right clavicle and tibia, attended with lancinating pains. Low diet and Fowler's solution were prescribed on the 13th of June. On the 15th the pains and soreness to the touch were removed; the nodes were much smaller; in five days more they had entirely disappeared, and the patient was perfectly well.

Dr. Parke prescribed it in the following cases:

— Taylor was admitted with chronic rheumatism, was salivated, and took guaiacum; painful nodes about the size of a walnut were discovered upon the upper and exterior side of the tibia, and the bone was enlarged. On the 14th of March, 1820, ten drops of Fowler's arsenical solution were given thrice a day. In two weeks the nodes and disease of the tibia were perfectly well.

— Urquhart, aged 45 years, was admitted with rheumatism, and very painful nodes on his extremities. On the 20th of May, 1820, Fowler's solution was administered. In about a week the pain was diminished, his general health improved, and in ten days the nodes were well.

Robert Williams, aged 26, was admitted on the 2d of February, 1820, with rheumatism, was salivated and relieved. A node of the size of a nutmeg was discovered on the clavicle. On the 19th of March, ten drops of Fowler's solution were ordered; the pain in two days began to abate, and in about two weeks he was well.

Two other cases have since occurred.

Extract of a letter from Thomas Cooper, M. D. Professor of Chemistry, in Columbia College, South Carolina, dated

"Columbia, May 5th, 1820.

"I AM much obliged by your letter. Dr. Davis, a physician of this place of long standing and great practice, has repeatedly exhibited the prussic acid in cases of phthisis, always with mitigation of the symptoms, but not yet with cure in any decided case: although in one or two, a cure may with some probability be attributed to this acid, but not certainly.

"The cough is allayed, the expectoration decidedly lessened, the colliquative sweats stopped, ease and sleep are induced, and in several cases the life of the patient has probably been prolonged.

"Hitherto he has given the medicine in the form of its condensation in water by Scheele's process. I forbear to say any thing of doses, in case of a remedy so liable to daily deterioration and decomposition. I proposed condensation in alcohol, and about ten days ago, I made some for Dr. Davis in that way, surrounding the receiver with a freezing mixture, which in this climate is absolutely necessary. As I did not in this experiment attend accurately to proportions, we tried it first on two dogs.

"To a dog of about 15lb. weight we gave at once 30 drops of the alcoholic solution of the kyanic acid. He drooped instantly, lay down, was *slightly* convulsed, and was dead in less than six minutes. We gave immediately after, sixteen drops to a lively dog of about 12lb. weight. He drooped, was strongly convulsed, and in about ten minutes stretched himself out apparently dead. He lay so for about six minutes, then recovered, and ran about as if nothing ailed him. The same dog was again tried with a smaller dose next day, but it produced the same symptoms without killing. He is alive and well.

Dr. Davis has since given it to I believe two patients, with the usual effect of mitigating the symptoms of the disease beyond all doubt: but one, a female, who was anxious to continue it from

the good she felt from it, was too far gone. She died I believe yesterday. It seems to me the strongest known sedative. I am now making it in this way:

“One oz. of red precipitate, boiled with 2 oz. of prussian blue, furnish 316 grains of chrystallized prussiat of mercury. This quantity of mercurial prussiat barely moistened with muriatic acid, and exposed to the heat of a spirit lamp in a small retort is decomposed: put into the intermediate tube a few small pieces of chalk; let the distilled prussic acid pass over this chalk, and condense it in one ounce measure of rectified spirits of wine surrounded by a freezing mixture of Glauber's salt, sal ammoniac and nitre, in the usual proportions. The receiver may be a 4 oz. vial, well corked with a pin run through the cork; 316 grains will furnish 24 oz. by measure of prussic acid gas, which by means of the freezing mixture can be condensed into one ounce measure of rectified sp. of wine, and will remain far more permanent than if condensed in water. When the flame of a taper applied to the pin hole, burns blue, the prussic acid vapour comes over; when the blue flame ceases, the whole is condensed; but the lamp heat should be continued a minute or two longer.

Of this alcoholic solution of prussic acid, two drops twice a day, is a full dose to begin with. It allays the cough, the fever, the night sweats, the expectoration of phthisis, by allaying the morbid irritability of the system. Dr. Davis has fully ascertained these points; but I cannot say that phthisis has yet been cured by it. Indeed a well characterized scrophulous phthisis, I consider as incurable.

Dr. Davis has exhibited sugar of lead in the morbid irritability of the stomach in high grades of bilious fever with great success; beginning with 3 grains at a dose, and repeating it or increasing pro re nata.

REVIEWS.

A Reply to certain oral and written Criticisms, delivered against an Essay on Lithotomy, published in the January Number of the American Medical Recorder. By Granville Sharp Pattison, Esq.

"Unnecessarily to mortify the vanity of any writer, is a cruelty which mere dulness can never deserve; but where a base and personal malignity usurps the place of literary emulation, the aggressor deserves neither quarters nor pity."—SHERIDAN.

THE unshackled discussion of political topics, is not more important to the public weal, than the unrestrained exercise of just criticism is salutary to the investigation of scientific truth. In both, liberty may degenerate into licentiousness; in both, public good may be forced to succumb to private advantage; and truth, however important, assassinated by traduction, may be scouted by popular odium, or silenced by the superciliousness of favoured ignorance.

It happens fortunately, however, for the interests of liberty and science, that the enemies of both, insidious as they may be in their attacks, are easily detected, and that their impotent efforts tend, in most cases, only to strengthen the prosperity of the causes against which they have been levelled. This observation bears with particular force on literary criticisms. If unjust, the individual aggrieved, has it in his power to appeal; and if he has truth to support him, there can be no doubt, that by the mightiness of her pleading, and the justice of public feeling, his doctrines, which have been carped at and abused, will be presented under a clearer aspect, more fully understood, and finally established.

Anonymous writing is usually employed for conveying to the public, critical observations on the works of letters and science. Usage, from time immemorial, has sanctioned, as consistent with the character of a gentleman, this kind of writing:—but honour demands, that these criticisms should be composed in the spirit of liberality, and published in regular journals. When written with a malicious feeling, for the purpose of injuring the character of a cotemporary, and, more especially, when published out of the regular channels which are employed for the dissemination of scientific communications, they lose all respectability, and merit the odious denomination of libels, equally inconsistent with the principles of honour, which should regulate the conduct of the gentleman, or the love of truth, which should direct the observations of the man of science.

In the justice of these remarks, I feel persuaded, that every man of principle and correct feeling, will agree. They have been suggested to my mind, from the consideration of certain oral and written criticisms, thrown out against the observations contained in the essay on the subject of lithotomy, I published in the January number of the *Medical Recorder*, and on the refutation of which I now propose to enter. But in order to render the subject more perspicuous, it may, perhaps, be judicious, to preface the following remarks, with a short history, of what occurred previous to, and immediately after, the publication of my essay.

The cause of truth, as well as the duty I owe myself, leave no alternative; my professional character must be defended or abandoned; but feeling shall be spared, and decorum observed, whether merited or not; truth being the only legitimate object in all scientific discussions.

In a conversation which I had with Dr. Physick, shortly after my arrival in America, I took occasion to mention the prostate fascia, as a discovery, and attempted to explain, from its connections, the causes of urinal infiltration. The Doctor, with that lively interest which he takes, in all that belongs to his profession, entered warmly into the subject, and stated to me, that, although not aware of any anatomical structure, which would modify the dangers of urinal effusion, he had been in the habit, for many years,

of introducing through the wound, a gum catheter into the bladder, with the view of allowing a free passage for that fluid; and that, since using this instrument, the success of his operations had been very great. It was agreed, that I should, the first opportunity which offered, dissect the fascia. Some weeks afterwards, being present at the examination of the body of a boy, whom the Doctor had attended, on account of false passages in the urethra, I took occasion, in his presence, and in that of Drs. Chapman, Dewees, M'Clellan, and several other gentlemen, to cut into the perinæum, and to give a partial view of the fascia. The parts were, however, in this subject, in such a gangrenous state, from urinal effusion, that the exhibition here given of the prostate connections, was far from being a satisfactory one. It was sufficient, however, to satisfy Dr. Physick as to the existence of a fascia, of which *he before knew nothing*, and drew from him a wish, that I would endeavour, at an early date, to dissect it in a subject, where the parts were in a state of health. The other gentlemen expressed with equal warmth, their conviction of the reality of my discovery.

Some days after this dissection, Dr. M'Clellan called with Colles's Treatise on Surgical Anatomy, and stated to me, that an individual who had been present at the exhibition of the fascia, had brought that work to Dr. Physick, and had endeavoured to convince him, that what I had claimed as a discovery, was not due to me;—that the fascia was clearly described by Mr. Colles, in the passage at which he had folded down the leaf. The Doctor's ideas of the connections of the fascia, being from the imperfect dissection given, only general, he was unable himself to say, whether the description to which his attention had been called by this discoverer of my making unjust claims, referred to the fascia which I had shown, or to something else. I had no difficulty in convincing my friend Dr. M'Clellan, that it referred to a part altogether distinct,—one, which was regularly and constantly described by anatomists.

Reading Mr. Colles's work, which was left with me, I found as I have stated in my essay, that that author does, in a different chapter from the one marked, make some observations on the prostate

fascia. So soon as I had satisfied myself, that I was not the first who had seen it, I called on Drs. M'Clellan and Eberle, and stated to them my conviction, that although Mr. Colles did not, in the passage to which Dr. Physick's attention had been called, allude to the prostate fascia, that still, that anatomist had seen it, though he "neither was aware of its connections, its importance, nor its uses." These gentlemen would not even agree with me in this, and still insisted, that the passage in Mr. Colles's work, which I supposed referred to the prostate fascia, and which Dr. Gibson read to his class, and which W. has published in the last number of the Recorder, was intended to describe some distinct part.*

Accompanied with Dr. M'Clellan, I visited Dr. Physick with the book, and with the most perfect candour, assured him that I was satisfied Mr. Colles had seen the fascia; and then went over with him that gentleman's description. Dr. Physick's observation was, *that it was so confused, it was impossible to understand exactly what the author meant*; and added, most unequivocally, that his having, or not having seen it, *would in no measure take from the honour which was due to me, for being the first who had brought it before the profession, in a highly interesting and important practical point of view.*

I heard nothing more of the prostate fascia until after the publication of my essay. Before it went to press, I took occasion in two different subjects to exhibit its connections, and demonstrate its existence in the presence of Drs. Physick, Parish, Hartshorne, and M'Clellan, and so satisfied were they of its presence, that they allowed me in my paper to make use of their names, with the view of satisfying the sceptical as to its existence.

Immediately after the publication of the January number of the Recorder, the subject of the prostate fascia was again brought on the

* Dr. M'Clellan in support of this opinion stated, that as Mr. Colles described his fascia as a layer of the triangular ligament, and as the levator ani muscle was interposed between that ligament and the one described by me, he must mean some other layer of fascia. I granted the correctness of my friend's objection, but insisted, that although in relation to this point, and others in the description, Mr. C's description was incorrect, that still I had no doubt but that the same fascia was meant.

tapis. And, although I had myself, been the first who had shewn that Mr. Colles had seen it, so little was that gentleman's book understood at this period, even by those gentlemen, who now declare, that his description is the clearest and most perspicuous possible, that they never attempted to take from me that discovery which they, incorrectly, conceived I had claimed, but endeavoured by childish jests and ill-tempered sneers, to make the world believe that the prostate fascia was a mere creation of my imagination, and that any man who could use a scalpel, could make a fascia just as easily, as a modeller in wax could make a nose. This pleasantry, though at my expense, as it could neither affect me nor change the structure of the perinæum, gave me no concern,—the thing denied, existed; and that truth, which they could not discern, was both capable of demonstration, and of vast importance to be known.

Unfortunately for the harmony of all concerned, Dr. Physick, when he came to the anatomy of the perinæum, regulated by a love of truth, demonstrated the "*prostate fascia*." This demonstration put an end to all mirth, and conveyed a most melancholy illumination to the minds of those who were before blinded. There was no longer a doubt that a fascia existed, which required for its formation, not a petty dissector, but the great "*horlogier de la nature*." But this was not the only enlightening effect produced by this demonstration. Mr. Colles's work was taken up anew, and, upon a re-perusal with minds illuminated, it was discovered, that his description was the clearest possible, and that I had been guilty of an unhandsome plagiarism, in claiming that, which belonged to another.

Having gone over this short historical introduction, I am now prepared to take notice of the different tangible attacks which have been brought forward against the essay. I shall confine my observations to Dr. Gibson's lecture, the anonymous letters published under the signature of Aristides, in Mr. Poulson's newspaper, and the criticism, which appeared in the last number of the Recorder.

Every Professor is bound, in the fulfilment of his duty, to guard his students, against the reception of that, which he conceives to be either false or pernicious, in doctrine, or practice: and

every man of science, is entitled through the media of scientific journals, to publish anonymous liberal criticisms, on the speculations of others in science. But neither the professor nor the man of science, who would wish to rank as a gentleman, is warranted to publish in a commercial newspaper, an anonymous and abusive criticism, against the doctrines of a cotemporary, that have been delivered in a manly and open manner, and published through a proper professional organ. It has been unjustly surmised, that I myself was the author of certain newspaper publications, which mentioned my name in flattering terms. But those who have assumed the liberty to make such insinuations, know them to be without foundation. As a man of honour, I declare, that I am exceedingly unwilling to have my name brought forward, either favourably or unfavourably, by anonymous newspaper writers; and that in the whole course of my life, I have never written or published a syllable to which I did not affix my signature.

As the whole profession have not had the advantage of hearing the remarks delivered by Dr. Gibson in his lecture, or of reading the criticisms of Aristides, it will be necessary, for me, before I attempt a refutation, to state shortly the observation of the professor, and of the anonymous newspaper writer.

I must confess, that the lecture delivered by the learned professor, was a most remarkable one. He began by stating, that having "*accidentally*" taken up Colles's Surgical Anatomy, a few days before, he had discovered that, that author had given a most luminous description of a fascia connected with the prostate gland. I felt pleased with the lecturer's zeal, in satisfying his class on this head, for I naturally concluded from the repetition he employed, and the anxiety he displayed in convincing his students that Mr. Colles had demonstrated the prostate fascia, that although he would give me no credit as a discoverer, still he would go along with me, in my views, as to the great practical deductions, which were to be drawn from the connections of this fascia. Judge of my disappointment when I heard the professor begin with equal warmth, after he conceived, that he had persuaded his auditors of the justness of Mr. Colles's claims, to assure them that the existence or non-existence, of the prostate fascia, was a

matter of not the slightest practical importance. 1st, It was of no consequence, because no gorget was made sufficiently large to cut the whole of the prostate gland; 2dly, it was a matter of no importance, for if this impossibility could be accomplished, still the division of the base of the prostate, would have no effect in producing urinal infiltration; and lastly, he inculcated, that urinal infiltration was not attended with danger.

I am aware, that some ill-tempered people, have supposed that the whole scope and bearing of the professor's discourse, was to persuade the students, that I had been guilty of an unhandsome plagiarism, from Mr. Colles, and like a silly plagiarist that I had purloined that which was perfectly useless. But, I must do the individual referred to the justice to state, that he observed in relation to my name, and claims, the most delicate and profound silence.

The newspaper critic, if we can believe him, is a philanthropist. His motto is rather a remarkable one.

"In nostros fabricata est machina muros."

Some of my learned brethren have been at pains to unravel its bearing, but I am quite satisfied that all their explanations are very far indeed, from tallying with the one the author wished it to convey. "Nothing short of the interests of humanity," could have induced Aristides,—honest man! "to come forward in a newspaper." Yet this noble and generous sentiment, makes him sacrifice his delicate feelings. Every man who reads this introduction, must be ready to exclaim, wonderful benevolence! Surely, the individual who has induced this excellent and humane gentleman, to write for Mr. Poulson's newspaper, must be some city pest! some abominable quack, who attempts to poison our citizens. This I am aware is the natural inference, which will be drawn from Aristides's introduction, but most certainly it is not a correct one. I am very fallible, and may most assuredly have been mistaken in my views. But certainly I have delivered these in a regular, professional and gentleman-like manner. And even allowing, that the sentiments which I have taught are erroneous, they are assuredly not of a character to desolate our population, and to call for the interference of a philanthropic Aristides, for their suppression.

It is said that the first sentence of an essay is the most diffi-

cult to compose, and, as the tenor of the letter seems to have nothing to do with humanity, but is written merely to satisfy the commercial public of Philadelphia, that Mr. Pattison is no discoverer and a man of little experience in his profession, we are warranted to suppose, that the author, being at a loss for an initial sentence for his letter, took one, which, in a happy moment of inspiration, he had composed for another purpose, and which, although foreign from the intention of his letter, he conceived too good, to be lost to the public.

In conclusion, the anonymous author, with the view of damning the unfortunate Scotsman, endeavours to rouse the national feeling of Americans against him, and finishes his *humane epistle*, with this imposing assurance, that the American public will not be imposed on with impunity, in other words that they are not to be "*humbugged*" by an ignorant foreigner.

The last criticism is before the profession, and it is therefore unnecessary for me to make any introductory remarks upon the observations which it contains. When I received at Mr. Webster's store, the number in which it is published, I felt so anxious, I must confess, to see what this regular criticism contained, that I took a peep into it, as I walked home. I was aware, that its author was my anonymous friend Aristides, and as his previous publication demonstrated at least the disposition to be ill-tempered and abusive, I expected that he would have written a very severe critique. It is a fact, which the readers of reviews must have remarked, that if there is any bitter observation, this is generally kept for the last paragraph. It makes a good finish, and sends the reader to bed well pleased with the humour of the author. The final paragraph was, therefore, the one I selected for my walking examination. It is certainly an attempt at sarcasm, but most assuredly a feeble one. It takes leave of me as a "*child of science*," assuring me that had the author had time after the preceding laborious production, he would here enter on my practical deduction. This it would doubtless be too much to expect from him, for one three months, and the public and myself, are therefore doomed to wait for an extension of the critic's labours, until another opportunity.

Having read the author's apology for his not having proceeded with the criticisms, I was certainly excusable, in going home in the belief, that the Recorder of which I had just become possessed must contain a very long review of my essay, and one in which there was a great deal of original matter. So soon, therefore, as I had composed my spirits with a cup of tea, I retired to my study, to spend the evening in its perusal. My astonishment was not a little, when I discovered that the paper contained only seven pages, and of these forty-three lines only were made up of original matter. I recollect once of hearing an old story of a professor who found it no easy matter to deliver his lectures, apologizing to his class, for not giving them a valedictory, nearly in the following words: "Gentlemen, I intended to have written you a very fine lecture, but to tell the truth, I am so morally and physically exhausted by my exertions, that I have been unable to accomplish it." I have no doubt, that W. was influenced by similar motives. He intended, I sincerely believe, to have written a very severe and spirited attack upon my opinions, but he became so morally exhausted in composing the forty-three lines which are original, and so physically fatigued by copying the six pages from Mr. Colles's work, and my essay, that his amiable intentions were frustrated.

The tenor of the criticisms, which have been brought against my essay, may be divided into two classes.

1st. Those which go to state that I have claimed as a discovery, that which belongs to Mr. Colles.

2d. Those, which attempt to disprove my practical deductions.

I shall divide my reply into two parts; in the first, I shall endeavour to refute those observations which belong to the former criticisms, and in the second place, attempt, in opposition to what has been brought against them, to establish and confirm those practical doctrines, which, I conceive, may be drawn from the anatomical structure of the perinæum.

I might give a very short answer, to those criticisms which accuse me of being guilty of claiming as a discovery, that which belonged to Mr. Colles. I have in the most candid manner possible, allowed in my own essay, all that is due to that gentleman.

I there, distinctly and clearly state that, "*that anatomist has seen it.*" In the garbled extracts which Aristides gives in the newspaper and Recorder from my essay, he wishes to convince the public, that, I do lay claim to the discovery, and in proof, makes quotations from the first part of the paper, to establish his assertions. I no doubt there employ the expressions quoted, "*a new fascia,*" "*this new fascia I named the fascia of the prostate gland.*" But Aristides must have been aware, that in using such terms, I am giving a history, a diary of my thoughts. The essay contains, in fact, a historical account of my thoughts on the subject of lithotomy. It is, distinctly, stated in the following words; "*in continuing the account of the diary of my thoughts in relation to lithotomy,*" a passage which immediately precedes the expressions quoted. I would ask the question, was it possible for me in writing a diary of my thoughts, to have used any other expressions? It was really, and truly to me, a discovery. It was considered as such, by all the professional friends, with whom I conversed on the subject in Edinburgh, London and Paris; and I still continued to believe it was so, until I read Mr. Colles's work in Philadelphia. I would demand, if there was any thing disingenuous, in my conduct after I read Mr. Colles's essay. His book had been pryed into by those, who were most anxious to take from me the honour of the discovery, but they were unable to understand that he had really seen the fascia. It was left for me to make that discovery, and when I did so, I was the first to proclaim it to Drs. Physick, Eberle and McClellan. Every just mind, must therefore allow, that my conduct in relation to Mr. Colles, has been most candid and honourable.

I trust, I have by these facts proven, that I have not claimed the fascia as a discovery, and shall now show, that had I been desirous of appearing before the profession, in the character of an anatomical discoverer, I might, with equal propriety have done so with those, who are universally acknowledged as such.

I suppose, when I state, that William Harvey is the discoverer of the circulation of the blood, and Gasper Asselius of the lacteal vessels, that it will be allowed, that I am making a correct statement. But is not the following assertion equally consistent with

truth, that Andrew Cesalpinus, described the circulation through the lungs, and that Realdus Columbus, La Faye and Garengoet, all wrote, more or less, distinctly of the movement of the circulation prior to Harvey; they understood it, without drawing clear physiological inferences from it, in the language of the biographer of Harvey,—“ Il étoit réservé à Harvée de développer cette vérité et l'on ne peut, sans injustice, lui refuser la gloire d'en avoir établi la preuve jusqu'à la démonstration.”

It may, with equal truth, be remarked in relation to the discovery of the lacteal vessels, that Hippocrates, Plato, Aristotle, Erasistrates, Herophilus, and Galen, had seen them; but Asseilius was the first who saw them physiologically; and it is he, therefore, who is honoured as their discoverer. If physiological inferences are necessary to constitute an anatomico-physiological discovery, certainly surgical deductions are equally required to establish an anatomico-chirurgical one; and I trust, that even W. will allow, that I have been the first to draw these inferences from the connections of the prostrate fascia.

Again, it would appear, that previous to the publication of my sentiments regarding the connection of the prostate fascia, none of the anatomists in Europe, to whom I had demonstrated it, were aware that such a fascia existed; and in America, no surgeon had ever thought of this connection. Dr. Physick, professor of anatomy in one of the first medical schools in the United States, a man who deservedly stands at the very head of his profession, in this country, allowed that he knew nothing of it before; and the professor of surgery in the same institution, does not, I believe, pretend that he was aware of it, until after Dr. Physick's demonstration, when he “*accidentally*” took up Colles's Surgical Anatomy.

W. states that in the 5th Number of Mr. Charles Bell's Reports, there is a plate given by him, to illustrate an essay by Dr. Gairdner, on the anatomy of the parts concerned in the lateral operation, and that the letter L designates “the fascia which surrounds the prostrate gland, and which afterwards covers the inside of the levator ani, and obturator internus.” I have not been able to obtain a sight of this number of Mr. Bell's Reports, and can say

nothing of the fascia alluded to by Mr. Gairdner. It is impossible for me to say from the quotation, whether it is intended for the one I have described or not. But this will in no measure militate against my claims, as the work quoted was not published until eighteen months after I had made public, as a discovery, the prostate fascia.

In answer to the next original sentence in W's paper,—“in fact, these parts appear to be spoken of, both by Mr. Colles and Mr. Bell, as matters of course, as things which have been long known, and to claim which, as discoveries, would undoubtedly seem in their eyes ridiculous in the extreme.” This is saying in very distinct terms, that not to be acquainted with the prostate fascia, is to be a mere tyro in anatomy and surgery.

I think this sentence has been written by W. without his being aware of all its bearings. I am satisfied, that no man, American or European, who is acquainted with Dr. Physick's professional character, will say, that he is ill-informed on the subject he professes, or the one which he practices; and from what I know of W. I can with perfect confidence assure the profession, that he is the last man living, who would wish the professor of surgery in the University of Pennsylvania, to be considered an ignoramus. If W., therefore, had only remembered that neither the professor of anatomy nor even Dr. Gibson were aware of the existence of the prostate fascia until I came to this country, I hardly think he would have insinuated, that every surgeon short of a fool, was familiar with it.

Every man who reads my essay with an unprejudiced mind, must be satisfied, that it is written, not for the purpose of laying claim to an anatomical discovery, but simply with the view of enforcing what I conceive to be highly important practical maxims and observations. In concluding it, I observed, “*the only claim I will make, and in this I am confident I will be supported, is, that until the present, no rational explanation has been given of the manner in which the urine is effused, and consequently, no operation has been philosophically proposed to prevent it.*” I shall not insult the understanding of the reader, by reasoning longer on this part of the subject. I feel perfectly satisfied, that every indivi-

dual whose good opinion is worth coveting, will be ready to award to me, much more than I have claimed.

The reader, in recalling to mind the history of my own experience, in lithotomy, as delivered in my former essay, will observe, that in the only cases where I ever had an opportunity of making dissections after death from that operation, I discovered gangrenous supuration betwixt the bladder and rectum, which, unquestionably, arose from the infiltration of urine into the cellular structure, which connects the bas-fond of that viscus with the gut; and that these operated as the cause of death, there can be no doubt. These facts are, I conceive, of themselves, quite sufficient to establish the correctness of my axiom, that one of the greatest dangers of the operation, is the effusion of urine. But in refuting the criticisms, which have been circulated against my opinions, I am unwilling to place any force, either in my own experience or assertions. I am desirous to confirm the correctness of my doctrines, by bringing forward to their support, passages from the works of those, who are, with justice, considered the great fathers of our art, and who, writing without a bias to this or the other maxim, must be considered as unprejudiced.

It will not be denied, I believe, that one of the greatest dangers of the *apparatus altus*, is the infiltration of urine, into the cellular substance which surrounds the bladder. If it be allowed, that the effusion of that fluid from an opening made into the bladder above the symphysis pubis, will, from its irritation, produce gangrenous inflammations, and suppurations in the cellular substance, with which it comes in contact, it can hardly be believed, that it will not produce precisely the same effects when allowed to infiltrate from a wound made into the shoulder of the viscus. Richerand, in his *Nosographie Chirurgicale*, Tome III., speaking of urine being effused into the cellular substance, mentions it in the following words: "*accident mortel toujours redoutable après la taille hypogastrique.*" And again, "*alors elles se feroient jour par la plaie supérieure s'infiltreroient dans le tissue cellulaire, et causeroient une gangrène mortelle.*" These observations of Richerand's, go to prove, that the effusion of urine above the pubis into the cellular substance, is an "*accident mortel,*"

one which gives rise to a "*gangrène mortelle*." From the next quotations it will appear, first, that urine produces the same effects when it infiltrates into the cellular substance, which connects the bladder to the rectum; and secondly, that this fluid will be apt to become effused, if the wound be continued either through the base of the prostate gland, or made through the prostate fascia, directly into the shoulder of the bladder.

Cheselden, in attempting to imitate the operation performed in Holland with the most extraordinary success by Rau, cut directly into the shoulder of the bladder, necessarily dividing the prostate fascia. The melancholy detail of the unhappy result, speaks volumes in proof of the dangers of dividing that fascia. Camper, in his *Anatomical Demonstrations*, thus records it. "*Cheseldenus, ut omnia tentaret, vesicam aqua hordei implebat, quantum ægri ferre proterant; dein vesicam incidebat, sed infausto successu, propter urinam inter vesicam et portas vicinas remorantum, unde gangrena, qua ex decem octo moriebantur.*" The operation of M. Foubert, which entered the bladder at the same point with the one described above, was equally unsuccessful. Sharp, in his *Critical Enquiry*, &c. details its fatal consequence; but he, like every author, who has considered the subject, gives an erroneous explanation of the cause. "Another great evil," he observes, "attendant upon a wound of the bladder in that part, is the want of a free egress for the urine, which insinuates itself into the cellular membrane, producing *abscesses* and *gangrenes*, which often prove fatal. Or, if they do not destroy, yet by lying on the rectum, produce a slough there, and thus form a communication between the bladder and rectum." To obviate this danger, M. Foubert invented, or rather revived the practice of Franco, and introduced a gum catheter into the bladder.

I should hope, that I have been enabled by the quotation of these passages to prove the justness of the opinion I have delivered, as to the great dangers which arise from urinal infiltration, and shall now bring forward some other quotations, which will further establish this doctrine, and at the same time prove, that although aware, of the great dangers which arise from infiltration, surgeons were not aware of the true reason, why a large wound,

which divides the basis of the prostate gland, and enters the shoulder of the bladder, is usually followed by them.

M. Sabatier, in his work "*De la Médecine Opératoire*," in attempting to explain the cause of urinal effusion, after those operations in which the shoulder of the bladder is cut, thus expresses himself. "Parce que l'écoulement des urines permet à la vessie de se contracter, et parce que la plaie de ce viscère cesse d'être parallèle à celle des graisses, et des tégumens. Ce défaut de parallélisme, augment la disposition aux infiltrations intérieures."

Desault, in his "*Ouvres Chirurgicales*," gives nearly a similar explanation of the cause of urinal effusion, "d'un autre côté, celui de ne pas établir de parallélisme entre l'incision extérieure des tégumens qui est oblique et celle du col de la vessie et de la prostate, que se trouve alors horizontale. De-là la possibilité des infiltrations par les obstacles qui les urines trouveront à s'écouler."

The last quotation, which I will give, in proof of my assertion, that there has been no correct statement given, until the present, of the cause of urinal infiltration, is taken from one of the very last essays which have been published, on the subject of lithotomy. Mr. Samuel Cooper, criticising the memoir of Scarpa, which recommends small wounds, says: "indeed, wherever urinal infiltrations do happen, I believe, that they proceed (not from the shoulder of the bladder being cut), but from a totally different cause, viz. from the incision of the skin being too small, and too high up, and from the axis of the internal part of the incision, not corresponding with that of the external wound."

Having thus established, in opposition to the assertion made against them, the two facts, 1st, That urinal infiltration is attended with great danger, and, 2dly, that the reason why it is more apt to follow an operation, where the wound made into the bladder is large, rather than one where it is small, has not until the present been explained. I come next to overturn the assertion of Aristides, "that my precept," not to cut the shoulder of the bladder, is in direct opposition, to the advice of the best authorities in Europe and this country.

From a rational anatomical explanation of the causes why urine is more apt to infiltrate in those cases, where the basis of the pros-

tate is cut, rather than in those, where it remains undivided, having never until the present been offered; I am prepared to grant, that, some of the most eminent surgeons, both in this country, and in Europe, have advocated a different practice from the one I have recommended. But, I feel assured, that I am supported by truth, when I state, that the practice of making large wounds into the bladder, has been followed with such unhappy consequences, that a majority of the very first surgeons, of this, and of the three preceding centuries, have, without any philosophical knowledge of the cause, been taught by the lessons of experience, to advocate the plan, of entering the bladder, by a small, in preference to a large wound.

Pierre Franco, one of the most eminent surgeons of the 16th century, and a man, who had, perhaps, more experience in lithotomy, than any surgeon of his time, thus expresses his opinion, in his work, entitled *Petit traite contenant Vue de parties Principales de Chirurgie, &c.* "Iaçoit que la moindre incision soit la meilleure;" and again, "Bref il est requis de tenir mediocrite." Brownfield, a man whose surgical knowledge is not disputed, argues, strenuously, against cutting the base of the prostate. "Nan tametsi aliter visum sit multis scriptoribus, fateor tamen, me non posse non putare valde perniciosam esse, partem, membranosam vesicæ sauciari, et si nihil aliud affert mali, fistulas orituræ maxime est verisimile." Scarpa, the great surgeon of Italy, assures us, that the shoulder of the bladder cannot be cut, "without the danger of occasioning urinary fistulæ, and gangrenous suppuration, in the cellular substance situated between the bladder and rectum;" and in another passage of his work, he thus writes; "if an incision was made through the base of the prostate, and into the orifice of the bladder, it would infallibly occasion infiltrations of urine, into the cellular substance, between the rectum and bladder, and subsequent gangrenous abscesses, fistulæ, and other severe accidents." Were I anxious, for further written proof, in support of my statement, I might quote from the works of Le Cat, Callison, &c. but this is, I conceive, unnecessary, recalling to my reader's mind, the fact, which I brought forward in my former essay, that two of the most eminent surgeons of Europe, Mr. Astley Cooper, and M.

Dupuytren, advocate, although on erroneous principles, small wounds. I trust, it will be allowed, I have made good my position, viz. that some of the very first surgeons of this, and the three preceeding centuries, although not aware of the anatomical causes, why urine should be more apt to infiltrate after an operation where the wound is large, rather than after one, where it is small, have been taught by experience, that such is the fact.

One of the assertions delivered by Dr. Gibson in his lecture, was, that no gorget was made sufficiently large, to cut the whole of the body and base of the prostate gland. The professor will, I trust, excuse me, when I state that this assertion of his, is perfectly gratuitous; that it is incorrect may, I think, be readily demonstrated. The anatomical fact, that the breadth of the prostate gland, very rarely, in a state of health, measures above seven lines, cannot be disputed. The statement, that many gorgets are made fourteen lines in breadth, is equally consistent with truth. Now, such being the case, I am at a loss to understand, how the ingenious professor will satisfy any man of common sense, that he can carry a sharp cutting instrument, fourteen lines in breadth, through the prostate gland, which, even in extreme cases, measures only seven, and still leave a part of it uncut. The assertion appears to us, to carry with it, such a palpable contradiction, that we are astonished it could have been entertained for a moment by Dr. Gibson. The prostate gland is composed of a very tough substance, one, which is not easy of division. It lies unsupported in the dead body, in *perinæo*, and consequently, when we carry a gorget into the bladder, in the subject, the two causes above mentioned, will operate, in allowing the instrument to enter, without making a division of the gland, proportionate to its breadth. Of this fact, any man may satisfy himself, and, I should hope, that Dr. Gibson, in delivering the assertion above quoted, was deceived himself, and was not desirous to mislead his students. I will take the liberty of recalling to his mind, the aphorism of De l'Ambert, that "there is no analogy between living matter, which is active, and dead matter, which is inert." When the instrument enters

the bladder, in the living body, there is no yielding of the prostate; it is not there unsupported, but is in fact fixed and pressed down, upon the sharp edge of the gorget, by the levatores ani muscles. It is self-evident, that if there is any disproportion now betwixt the extent of the wound and breadth of the gorget, that this will be in favour of the size of the wound.

The correctness of the second assertion of the professor, that even allowing, that it were possible to cut the basis of the prostate gland, that still it would be a matter of no moment, and would have no effect on urinal effusion, is equally erroneous, and can be proved as such, in a very few words.

I need not recapitulate here, the description of the connection of the prostate fascia given in my former essay. I have, since I came to this country, demonstrated it to above four hundred persons, and all of those, with whom I have had an opportunity of conversing, have given their hearty and cordial assent to my two-fold position, "that the prostate fascia separates the perinæum from the cavity of the pelvis, and that the basis of the gland remaining uncut, *it is physically impossible, for one drop of urine to infiltrate into the cellular substance, which connects the bladder to the rectum.*" It may be said, that it is merely my assertion; it is true, but it is an assertion, which, if false, I would not be likely to make, as it would be refuted by the four hundred individuals, who have been present at different times, when I made the dissection of the perinæum, and one, the correctness of which, I shall at all times be happy to demonstrate on the subject, to any of my professional brethren, who may desire it.

It may appear strange, if the verity of what I have advanced be substantiated, that the professor of surgery, could have made an assertion so directly opposed to it. I trust, however, I shall be able to explain this, without suspecting him of conduct so unworthy of his situation, as that of intentionally deceiving his pupils. There was something upon the table, which he told us, had the fascia shown upon it. If it was really dissected, it did not come under the sphere of my vision, and I am, therefore, charitable enough to suppose, that, as the gentleman who first brought Mr. Colles's work to Dr. Physick, mistook his description

of another fascia for the one described by me, Dr. Gibson was equally misled, by dissecting another layer of fascia, instead of the one connected with the prostate gland.

I have thus gone over, in order, the different charges which have been advanced against my essay. I fondly, and confidently hope, that I have clearly refuted them to the satisfaction of my professional brethren. The task which I have had to perform, has not been a difficult one; some of the criticisms may have been marked by ill-temper, but none of them, assuredly, by professional erudition, or mental acuteness. I have really felt sorry, that the critics have not brought forward some ingenious arguments against my doctrines, for had they done so, a more elaborate train of reasoning would have been required from me, for their refutation; and as the brightness of truth is like the brilliancy of the diamond, the more it is examined, the more refulgent; so their criticisms, if of a superior character, would only have tended to establish and confirm my opinions. So satisfied do I feel of the justness of this observation, that I am unwilling to take leave of my critical friends, without assisting them, with, at least, one observation, which certainly carries with it a much more powerful inference against my maxims, than any of those puerile objections, with which their minds have furnished them. It has been stated, that Cheselden in his third, and most successful operation, carried a knife into the bladder, behind the prostate, and cut the body of the gland outwards; now if this statement was a correct one, it would, of itself, be sufficient to tear up the very root of my reasonings. If Mr. Cheselden, in his most successful operation, cut the basis of the prostate gland, urinal infiltration could not be one of the great causes of danger, and consequently, cutting, or not cutting that aponeurotic expansion, which, entire, renders the infiltration of urine impossible, would not be a matter of such importance as I have insisted on. I am aware, that many believe, that Mr. Cheselden did perform his last operation in the manner which I have described, but I feel persuaded, that although this is a generally received opinion, it is not consistent with the truth. 1st, I think it can be disproved from the difficulties opposed to the perform-

ance of such an operation, and I have no hesitation in asserting, that was a dextrous surgeon to operate on twenty living subjects, and proceed with the view of executing his operation on the principles which were supposed to regulate Mr. Cheselden, that not in more than one out of the twenty, would the wound extend through the base of the gland, although he might himself believe, that this was accomplished in every instance. On the dead body, the operation may be much more easily accomplished; but even here, experiment will demonstrate, that supposing you have divided the gland, and really done so, are two very different things.

2dly, I conceive, that the fact, that Mr. Cheselden only divided the whole body of the gland, leaving the basis entire, is settled by the account of his operation, which is published by his pupil and assistant, Mr. Sharp. This gentleman, in his *Treatise on the Operations of Surgery*, thus speaks of it:—"This wound (external) must be carried deeper between the muscles, till the prostate can be felt; when searching for the staff, and fixing it properly, if it has slipped, you must turn the edge of the knife upwards, and cut the whole length of the gland, from within outwards." From this, it is evident, that the whole length only of the gland was divided; and that its base was left uncut, is satisfactorily proven by another remark of Sharp's; "there must be laceration, as in the old way (*apparatus major*); but in the one case, the laceration is small, and made after a preparation for it, by an incision; and in the other, all the parts I have mentioned are torn, without any previous opening." Thus we are in the plainest language informed, that the only difference betwixt this method and the Marian one is, that a small wound prepares the parts for dilatation; that this is required in both. If the base of the prostate and shoulder of the bladder were divided, there could be no occasion for laceration.

From these facts, I do conceive, that we are perfectly warranted in supposing that in the great majority of the instances, where Mr. Cheselden performed his operation, he left the base of the gland uncut, that in a few of them, it was divided, and that the fatal cases were the ones where this occurred.

I shall conclude this Review by calling my readers' attention to Rau's operation, as I conceive it affords strong and unanswerable evidence, in support of the justness of my doctrines. It is well known, that the celebrated Dutch lithotomist, made a secret of the parts he divided, and as death never occurred after any one of his operations, although he cured fifteen hundred patients, no opportunity was afforded the profession, of ascertaining, by dissection, the nature and extent of his incisions. I think, however, from the accounts which are handed down to us of his method, by those who saw him operate, that he was guided by exactly similar principles with those, which we have endeavoured to inculcate. Sabatier, in his "*Médecine Opératoire*," thus speaks of his cutting into the bladder: "Il incisait sur la sonde et pénétrait jusque dans la vessie. Alors il donnait la sonde à tenir, prenait le lithotome de la main gauche, et glissait à sa faveur un conducteur mâle. Le lithotome ôté, ce conducteur servait à en introduire un femelle, et il achevoit l'opération comme il a été dit en parlant du grand appareil." Here, we have an operation recommended, the principles of which, coincide exactly with the one I have advised; an operation which, although executed on fifteen hundred patients, was never followed by a single death. How very different is the success of the most celebrated lithotomist of the present day, to that which attended Rau's operation? From the records of the Norfolk and Norwich Hospitals, we learn, that the number of deaths, for the last seventy years, have averaged four in twenty-nine. Rau had not one in fifteen hundred!! It is not fair to get over the difficulty by explaining the cause of the difference of success, upon the principle, that the cold phlegmatic constitutions of the Dutch are more favourable for the operation than the warm sanguineous English. But it is consistent with the principles of sound logic, and pure philosophy, to elucidate it, by showing, that in Rau's operation, a division of the base of the prostate gland could never happen, and consequently, that *urinal infiltration was, in all his cases, rendered physically impossible.*

A System of Chemistry for the Use of Students of Medicine. By Franklin Bache, M. D. &c. 8vo. pp. 624. Philadelphia, 1820.

PLACED in the midst of the vast amphitheatre of nature,—surrounded by an order of things, with which, both as rational and physical beings we stand in intimate relation; we are strongly invited by curiosity and interest to cultivate an acquaintance with the objects under whose constant influence we exist.

The physician of enlarged and liberal views cannot confine his attention, merely to the “vital frame.” He sees it endowed with faculties which subject it to the constant, obvious and mysterious influence of surrounding inanimate nature; he sees morbid phenomena arise in living bodies from the agency of those which are dead, and it is to the latter that he resorts for agencies to remedy the diseases of the former. He is, therefore, naturally directed to the investigation of those inanimate causes, as well as of the laws of vitality. Hence chemistry becomes to the physician an important science; for although every department of knowledge may be more or less tributary to medicine, yet chemistry, holding the keys to the inner apartments of the temple of nature, is, of all other sciences, the one which is most closely allied to that of medicine.

The importance of this science to medicine, is indeed so generally acknowledged, that in every institution where medical science is taught on a comprehensive plan, chemistry forms an essential branch of instruction. Useful, however, as chemistry unquestionably is to the physician, yet, so vast is its extent, so intricate and abstruse its nicer and more delicate details, that the student of medicine, who after all must be chiefly engaged upon the more immediate departments of his studies, cannot, nor ought he, perhaps, endeavour to, obtain a full knowledge of the stupendous fabric of this science. He is obliged, therefore, from having his attention chiefly applied to the more important objects of his professional studies, to dwell only upon such parts of this science, as have some bearing upon his profession, and to remain

satisfied with a general knowledge only of those parts of it, which are more remote from his principal object.

This being obviously the case, it must be a matter of much importance to the student of medicine, to be furnished with an elementary work on chemistry, which should contain all that is connected with his professional studies, interwoven with a perspicuous and succinct view of the elements of the science, and unincumbered with its abstruse and irrelevant parts.

Such a book, is the one which we have before us. It was expressly composed for the use of the medical student; and from a very attentive examination of it, we are of opinion, that it is extremely well calculated to answer the purpose for which it is intended.

Although constructed after the general plan of Thomsen's work, we find in this book many judicious deviations both in arrangement and nomenclature from that and other chemical authors.

Contrary to what is usual, Dr. B. commences his book with the consideration of ponderable instead of imponderable matter. After this he considers the attractions, or general laws and relations of matter.

The usual mode of beginning works of this kind by treating of the attractions, or general chemical relations of matter, before matter itself is considered, would appear, we think, less natural than the opposite arrangement. It certainly is more consonant with the natural progress of the mind in the acquirement of knowledge, to consider the object before its attributes, than first to consider the attributes or properties, and then their substances. Besides, all that can be said upon the attractions or chemical relations of matter, must, as the author observes, be the result of the generalization of chemical facts.

In the arrangement of undecomposed ponderable bodies, Dr. B. adopts Thomson's primary divisions of *supporters of combustion*, *incombustibles* and *combustibles*. In his subdivisions of the combustibles he deviates materially from Dr. Thomson. He has added a new genus to this class of substances, which he denominates acidifying combustibles. Hydrogen is the only substance that stands under this genus.

Although it is still somewhat doubtful whether hydrogen deserve to be considered as an acidifying substance, yet the reasons which are given for thus separating this substance into a distinct genus are, we conceive, sufficiently strong.

"There are a number of acids which agree, as to constitution, in no other particular than in containing hydrogen. These are hydrochloric (muriatic) acid, hydriodic acid, hydrosulphuric acid (sulphuretted hydrogen), hydroselenic acid (selenuretted hydrogen), hydro telluric acid (telluretted hydrogen), hydro fluoric (fluoric) acid, and hydro cyanic (prussic) acid. It is for this reason, that I have put hydrogen in a class by itself, and ventured to call it an acidifying body. The term *acidifying* is not meant to convey the idea of any exclusive agency of the hydrogen in the production of the acid properties, in the particular acids above mentioned; but it is used as being convenient, and best suited to express the idea.

"Besides, hydrogen does not stand well along side of the bodies, with which Dr. Thomson has associated it in his first genus. Excepting hydrogen, all the bodies of this genus form acids with oxygen (assuming Dr. Thomson's opinion respecting silicium), and agree very well in this particular. Hydrogen, however, forms no acid with oxygen; while with sulphur, one of the substances also included in the genus, it forms a compound having acid properties."

The second genus of the class of undecomposed combustibles is entitled *acidifiable* combustibles. The definition given to this genus by the author, differs from that which is given by Dr. Thomson to a corresponding genus of the same title. Dr. Thomson's genus of acidifiable combustibles "includes such bodies as form acids by uniting with the supporters of combustion or with hydrogen." That of our author includes such bodies only as form acids, and in no instance salifiable bases by uniting with oxygen. Hydrogen is therefore excluded from this genus and placed in a separate one.

We find also several other judicious improvements upon Thomson's formation of this genus: thus, silicium and tellurium, which stand we think very incorrectly, in Thomson's genus of *acidifiable*

combustibles, are excluded from the corresponding genus of our author, because the proofs of the oxide of silicium (silex) being an acid are wholly unsatisfactory, and because although tellurium forms an acid with oxygen, it also forms a base with this supporter. Chromium, molybdenum, tungsten and columbium, which stand in Dr. Thomson's third genus, entitled, bodies capable of forming with oxygen *imperfect acids*; are introduced into this genus by Dr. Bache, because they form acids with oxygen and never salifiable bases. The third genus is entitled, *intermediate combustibles*. The meaning which Dr. B. attaches to the term *intermediate combustibles*, differs entirely from that which Dr. Thompson applies to the same term.

"The title of my third class is adopted from Dr. Thomson, but by intermediate combustibles, I do not mean the same thing with this author. He understands by intermediate combustibles, such bodies as form 'by their union with the supporters of combustion imperfect acids, or substances intermediate between acids and alkalies.' Under the acceptation in which the term is here taken, it designates those combustibles which, by union with oxygen, form compounds, which act the part of acids and of bases, in different cases of combination. Hence the reason that chromium, molybdenum, tungsten and columbium are carried into the second class in this work; since the oxides of these bodies, which enter into the constitution of salts, always take the part of acids, and in no case of salifiable bases. Titanium also is excluded from the class of intermediate combustibles, where Dr. Thomson has placed it, as not answering to the meaning of the term, as here adopted. Under the particular acceptation in which the title *intermediate combustibles* is here taken, I retain but one body, placed under this head by Dr. Thomson; and that is antimony. It is because this metal forms compounds with oxygen, one of which performs the part of a salifiable base; while the others (antimonious and antimonie acids) perform the part of acids. Tellurium, which was excluded from Dr. Thomson's genus of acidifiable combustibles, is inserted here; because the only compound which it forms with oxygen is sometimes an acid and sometimes a salifiable base."

The fourth genus of undecomposed combustibles is entitled *basifiable combustibles*. The term *basifiable* is new, and peculiar to our author, and appears to us, to be an exceedingly good one. The term *alkalifiable combustibles*, adopted by Dr. Thomson to designate the same class of bodies, is much less applicable here, as a generic term. For all the bodies which are placed in this genus, do not form alkalies by uniting with the supporters of combustion, yet all of them form salifiable *bases*, that is, are *basifiable* by uniting with supporters. Hence, Thomson was obliged to give the following inconsistent definition to his generic term *alkalifiable combustibles*; (i. e.) "bodies forming *alkalies* or *bases*, by uniting with the supporters of combustion."

This genus is divided into three subdivisions: 1st, *Alkalifiable combustibles*; 2d, *Combustibles forming earthy salifiable bases*; and 3d, *Combustibles forming salifiable bases, having neither alkaline nor earthy properties*.

The atomic theory is introduced immediately after the undecomposed bodies. The consideration of the compounded bodies follows the account of the atomic theory.

The compound bodies are treated of under the four heads, *salifiable bases, acids, salts, and unsalifiable compounds*. In his subdivisions of the salifiable bases, Dr. B. does not follow the usual nomenclature of authors. He employs the expression *alkaline salifiable bases*, to designate the compounds usually distinguished by the separate appellations of *alkalies* and *alkaline earths*, retaining no division corresponding to the latter. To the bodies usually called *earths*, he assigns the name of *earthy salifiable bases*; and to the compounds, which are generally denominated *metallic oxides*, he applies the expression of *salifiable bases, not alkaline or earthy*.

This nomenclature is precise, and indicates with clearness, the general character of the substances to which it is applied.

The *acids* are arranged on an entirely new plan by our author. His arrangement of this class of bodies is founded upon the relations of their various bases to oxygen and hydrogen, the only two acidifying principles known. Upon such principles he forms the following five genera of acids:

1. Acids, whose bases form acid compounds with oxygen or hydrogen indifferently.
2. Acids, whose bases form acid compounds with oxygen only.
3. Acids, whose bases form acid compounds with hydrogen only.
4. Acids, in which neither oxygen nor hydrogen is present, or of irregular constitution.
5. Acids, in which oxygen and hydrogen are both present.

"By the first class of acids the fact is clearly shown that chlorine, iodine, sulphur, selenium and tellurium, are acidifiable by combining with either oxygen or hydrogen. The second class contains those acids whose bases are acidifiable by oxygen only. These acids agree exactly in chemical constitution, and stand very well together. The third class contains the hydro-fluoric (fluoric) acid, and hydro-cyanic acid. The former has an undecomposed base, the latter a compound base. Both these bases may be considered as acidified by hydrogen. The fourth class appears to contain a very proper association of acids. They are called acids of irregular constitution, and they are certainly irregular with reference to any theory. Acids of the fifth class are those in which oxygen and hydrogen are both present. They are all, except the two first, triple compounds of carbon, hydrogen, and oxygen."

If the facts upon which this arrangement is founded were conclusively established, it would undoubtedly be a very correct and useful subdivision of this class of bodies. But it is certain, that some doubt still exists in relation to the acidifying property of hydrogen.

The salts are thrown into as many classes as there are salifiable bases, formed from distinct radicals. The author does not employ the name of the radical which furnishes the bases, in distinguishing the different classes of salts. Hence, he does not use the expressions of *salts of iron*, *salts of lead*, *salts of mercury*, &c. "When the chemist speaks of the salts of potash, the expression is strictly correct, and designates all those salts into which potash enters as a salifiable base. When, however, he speaks of the salts of iron, the mode of expression is devoid of the same precision;" for it is applied to salts not formed directly by the com-

bination of an acid with iron, but to salts formed by the union of an acid with *oxidized* iron. To avoid, therefore, the perplexities of an erroneous nomenclature, our author proposes to distinguish the metallic salts by the additional epithet *oxidized*. "The expression of *oxidized iron*, may be received as a generic one, including under its meaning, the two distinct oxides of iron (protoxide and peroxide of iron)." He has, therefore, appropriated to the salts formed from the protoxide and peroxide of iron, the title of *salts of oxidized iron*, instead of the usual one of salts of iron.

As this nomenclature is unquestionably more precise than the one in common use, it must be considered as no unimportant innovation. A correct and natural terminology assists the student very much: it supplies important auxiliaries to the memory.

The fourth division of compound ponderable bodies is entitled *unsalifiable compounds*; under this head our author arranges alcohol, ethers, volatile oils, fixed oils, spermaceti and soaps.

After having treated of ponderable matter, he proceeds to the consideration of imponderable bodies.

The subject of light is not considered at much length. Heat, however, being more immediately a chemical agent, receives all the attention which its importance merits. Upon this subject our author does not exactly follow the beaten path, but enters into some new, and, we conceive, ingenious speculations, evincing a deep acquaintance with his subject.

"The view which is here taken of this subject," says our author, "supposes that caloric is primarily put in motion by all causes which influence the capacities of different bodies. When the capacity of a body is diminished, it possesses a distributable excess of caloric over surrounding bodies, and is said to be heated; when the capacity is increased, the distributable caloric of a body becomes defective; the body itself absorbs caloric from surrounding bodies, and is said to be cold. Hence, the causes of the motion of caloric may be divided into those which give bodies a distributable excess of caloric, and those which render their distributable caloric defective."

To these views we entirely subscribe. When sulphuric acid and water, at a certain temperature, are mixed together, the tem-

perature of the mixture rises at once, considerably above the point possessed by each of the articles separately. This can only depend upon the mixtures possessing a less capacity for heat than did the articles in their separate state.

We do not, however, agree with the author, when he says—

“ Nothing else is meant by equality of temperature between any two bodies, than that the heat which they contain is not impelled to move from either to the other.”

According to this view of the subject, then, heat is radiated from a body only, when another possessing a lower temperature is in its vicinity; and when bodies have arrived at an equality of temperature, there is no more radiation. According to our notions upon this point, and we believe they are those which are generally admitted by chemical writers, there is a constant and never ceasing elimination of heat from bodies, whatever may be their relative temperatures. Mr. Prevost's* opinion relative to this subject, seems to us quite satisfactory. According to him caloric is a *discrete fluid*, each particle of which moves with enormous velocity when in a state of liberty. “ Hot bodies emit calorific rays in all directions; but its particles are at such a distance from each other, that various currents may cross each other, without disturbing one another, as is the case with light. The consequence of this must be, that if we suppose two neighbouring spaces in which caloric abounds, there must be a continual exchange of caloric between these two spaces. If it abounds equally in each, the interchanges will balance each other, and the temperature will continue the same. If one contains more than the other, the exchanges must be unequal; and by a continual repetition of this inequality, the equilibrium of temperature must be restored between them.”

The following explanation, given by our author, of the manner in which water freezes, is ingenious, and deserves to be particularly noticed.

“ It may be curious to enquire, in what peculiar manner heat leaves water, with the effect of making it solidify. It cannot be,

* *Recherches sur la Chaleur.* Vide Thomson's Chemistry, vol. i. p. 61.

that, when water is at the freezing point, the cooling cause acts exclusively upon a small portion of it, and converts it into ice, by abstracting 140° of caloric, by no means; for the 140° of caloric, which appear, when water is converted into ice, is the consequence, and not the cause, of the freezing. It appears to me, that the phenomena, which takes place in the freezing of water, cooled down below the freezing point, furnish the basis for a true explanation; since it is reasonable to suppose, that, during the ordinary freezing of water, the same happens on a small scale, which occurs in these other cases on a large one. When water is cooled down to the freezing point, it must suffer a small decrease of temperature before it can freeze. Now, I suppose, that when the first portion of ice is formed, it is exactly so much as is sufficient by means of the heat it gives out in the freezing act, to raise the whole (both water and ice), to the freezing point. If the cooling cause continue to act, the temperature of the remaining water sinks again, and again so much ice is formed as is sufficient, by its heat evolved, to raise the whole once more to the freezing point. In this manner, by the water repeatedly falling below the freezing point, and being as often raised to the same point, by heat evolved during the freezing of a portion of it, the whole at last becomes converted into ice".

After the consideration of heat and light, the author treats of the general principles of chemical action.

The second part of the work is taken up by "the chemical examination of nature." In this part, mineralogy is entirely omitted, as being in itself a science too exclusive and remotely connected with the main object of the work, to be introduced with advantage.

Although the author has given the chemical preparation, and noticed the medicinal properties of all the chemical medicinal preparations worth noticing in a work of this kind; yet we are of opinion, that he should have bestowed a greater share of attention upon the medical parts of his work, than he evidently has done.

Upon the whole, however, we are much pleased with the work. It is not, as we supposed, when we first took it up, a mere compilation. We find in it many marks of original thinking, and an independence of opinion, altogether creditable to the author.

To the editor of the American Medical Recorder.

SIR,

"It would have been unnecessary, nay, perhaps improper to notice," the Review of the History of Scutellaria, published in the Medical Recorder, vol. iii. p. 267, "did it not present so imposing an appearance."

"The mass of evidence," says the reviewer, "which it professes to adduce, and the numerous cases which are therein reported in *favour* of the antidotal and curative virtues of the scutellaria, render it incumbent on us to examine this pamphlet."

If the reviewer had read the pamphlet with half the impartiality, with which it was written, he would have perceived that it is precisely what the title page declares it to be, viz. A History of the Introduction and Use of Scutellaria. He would also have discovered that the writer had not *once* expressed, nor ever hinted at his own opinion of the antidotal virtues of the plant; and furthermore, if any character has been given of it, certainly this was not a very exalted one; for in page 6 of the pamphlet it is written, "besides, on a reference to the many *nostrums* which have been celebrated for preventing hydrophobia, we do not find that scutellaria has been used."

If the reviewer had adverted to the circumstances under which the pamphlet was written, and the society before which it was read, he would not have called it a "medical work."

It may not be irrelevant to remark, that at the time this tract was produced, the scullcap was trumpeted through the newspapers from Maine to Louisiana as a sovereign remedy for hydrophobia, with bold assertions that hundreds had been cured by it, and that it had never failed of success in a single instance; that the medical profession had been abused for withholding their approbation from the article, and for neglecting to use it. Under these circumstances the writer made known his wish to collect all the facts and cases relative to the use of scutellaria, in hydro-

phobia; that the public might be enabled to judge how far it was entitled to their confidence.

The whole history of its use, is believed to have been developed; and in every case the authority is quoted. It would have been presumptuous in the writer to have obtruded his opinion on the public, as he had no personal knowledge of the use of the article, nor any information which he did not communicate.

Reviewer.—“Whether it will add any weight to his (Dr. Lawrence Vanderveer's) testimony, that he had prescribed the scullcap in four hundred cases of a disease which physicians, of advanced years and extensive practice, seldom have an opportunity of seeing, we leave the reader to judge. *Credat Judaeus Apella, non ego.*”

Pamphlet, page 6.—“It is asserted, however, that he (Dr. L. V.) had administered it (scullcap) to about four hundred persons; in *none* of whom, excepting the following, did symptoms of hydrophobia appear.” Then follow the *three* cases in which symptoms of hydrophobia appeared. This reviewer is a very Falstaff, to magnify *three* cases of hydrophobia into “four hundred of that disease.”

Reviewer.—“We are next told that Dr. Thomas Steel was reported to have employed the *scutellaria* with success. But we are furnished with no case which occurred in his practice, except that of a boy, who died of hydrophobia under his care.”

This is a manifest *misrepresentation of every fact* respecting the case. The boy was bitten, as we learn from the Medical Repository referred to in the pamphlet, on the first of April 1801, and on the afternoon of that day, was sent to New Jersey. He was *supposed* to have taken *scutellaria*, and *probably*, from Dr. Steel. In less than a fortnight, the boy returned to New York in perfect health. On the 10th July following, he was attacked with hydrophobia, was attended by Drs. Rodgers and Miller, and died on the fifteenth.

Reviewer.—“It is really surprising that an author can expect to receive the confidence of the public, who is capable of advancing such feeble testimony in support of so great an innovation in medical practice.”

It is confessed, that the writer *might* have touched up these cases in such manner as to have made them appear much *stronger* than they do in their present form.

It is really surprising that a reviewer should expect to receive the confidence of the public, who is capable of perverting every fact in a case; and then mistaking the bearing of the testimony; for, if this case proves any thing, it certainly shows the inutility of scutellaria.

Reviewer.—“Dr. Spalding informs us that Mr. Coleman, the editor of the New-York Evening Post, who has been very vociferous in lauding the virtues of sculicap, has repeatedly called on the public to make known a single well attested case of its failure; and triumphantly adds, that no case has yet appeared.”

Let the pamphlet speak for itself; page 29, in a note at the bottom are these words: “Mr. Coleman has repeatedly called on the public to communicate to him a single well attested case of the failure of the scutellaria; but no such case has yet appeared.” Where is the triumph? what the evidence of exultation?

Reviewer.—“We have thus presented our readers with a brief review of the evidence contained in Dr. Spalding's pamphlet, in favour of the prophylactic and sanative properties of scutellaria; and we trust that we have made it appear, that the evidence is altogether unsatisfactory and inconclusive.”

A wonderful discovery indeed! when we consider, that neither in the pamphlet, nor since its publication, has the author intimated that the evidence was satisfactory; but always declared that there is not sufficient testimony to convince any medical man of the antidotal powers of the plant; besides, the reviewer, many months before the writing of his critique, repeatedly heard the author declare, both publicly and privately, his opinion of the scutellaria.

S.

FOREIGN PAPERS.

(From the London Medico-Chirurgical Journal, for April, 1820.)

I. Essays on Hydrocephalus Acutus; or Water in the Brain. By J. Cheyne, M. D. F. R. S. E. &c. One Vol. Octavo, Second Edition, 168 pages. London, 1819.

II. Hydrocéphale Arguë essentielle, et Symptomatique. Par M. Vaidy. *Dict. des Sciences Medicales.*

III. Memoire sur L'Hydrencéphale, ou Céphalite interne Hydrencéphalique. Par J. F. Coindet, M. D. Medecin en Chef des Hospices de Genève, &c. Octavo, pp. 292. Genève, 1818.

IV. A Statement of the Early Symptoms which lead to the Disease termed Water in the Brain, &c. with an APPENDIX to ditto. By G. D. Yeats, M. D. F. R. S. Fellow of the Royal College of Physicians, London. 1817 and 1819.

V. Of Apoplexia Hydrocephalica, or Hydrocephalus Internus. By John Cook, M. D. F. A. S. Fellow of the Royal College of Physicians, &c. &c.

[Treatise on Nervous Diseases, Chap. viii.]

THE ancient physicians had but a very imperfect notion of Hydrocephalus. Hippocrates does not mention it, and Areteus merely enumerates it among the different species of dropsy. Galen is more particular, and divides hydrocephalus externus into four spe-

cies, which division is followed, with some additions, by *Ætius*, and others. It is curious, however, that the ancients describe *hydrocephalus externus* as very common in their time, whereas it is now an extremely rare occurrence. Could this be owing to the violence offered to the heads of infants by the rude and unskilful midwives of old? *Rhazes* is the only one of the *Arabians* who hints at *hydrocephalus internus* when speaking of a morbid enlargement of the head, from an accumulation of aqueous fluid in the interior of the cranium. It is only among the moderns, who have been aided in their researches by the light of morbid anatomy, that we are to look for any precise and certain knowledge respecting this important disease.

We shall divide *hydrocephalus actus* into two species, the idiopathic and the symptomatic.

1. *Idiopathic Species.* The definition of this species might be briefly conveyed in the following terms. "*A rather sudden effusion of serous fluid into the ventricles, or on the surface of the brain, from the tunica arachnoides or pia mater, idiopathically affected.*"

Our own countrymen led the way in the investigation of this disease. Yet, about ten years before *Whytt* and *Fothergill* published their opinions and observations on dropsy of the ventricles, *Meyserey*, in a work on the diseases of soldiers, gives no bad description of the complaint, under the name of *brain fever*. In the writings of *Pringle* and *Huxham* also, may be found scattered hints of a similar tendency; but in all these, the effusion was considered as a termination, accompaniment, or consequence of some other disease. *Cullen*, for instance, denominates it a variety of apoplexy, "*Apoplexia Hydrocephalica*," and *Pinel*, in the first edition of his *Nosographie Philosophique*, considers *hydrocephalus actus* as a variety of *brain fever*, in imitation of *Chardel*. But in the last edition of his work, this author restores the disease to its proper rank, as an idiopathic affection, and places it in the class of Dropsies.

Causes of Hydrencephalus Acutus. Let it be remembered, that we are here speaking of the *idiopathic species*, which will account for the limited range of occasional and exciting causes. Youth appears to be what we may term the predisposing period; parti-

cularly that which intervenes between the two dentitions. Peculiarities of *hereditary organization* are now well known causes, especially the strumous diathesis, characterized by a delicate, irritable, and often beautiful frame of body, with a corresponding acuteness of intellect, and liveliness of disposition.

Among the more tangible *exciting* causes of the complaint, we may reckon injuries or commotions of the head by blows or falls. We have seen several instances of this kind, and we suspect that it is more frequently the case than is believed. Violent mental emotions, by throwing the heart into inordinate action, and thus pouring extraordinary currents of blood on the delicately organized brain, conduce to the development of the disease in the predisposed. The neglect or maltreatment of children among the poor, where they are left crying, almost to convulsions, for hours together, must lead, in many instances, to effusion in the brain. The suppression of a spontaneous or habitual evacuation from the system, as of nasal hæmorrhage, diarrhœa, or eruptions about the head in dentition, form a fruitful source of irritation on the coverings of the brain, and consequent effusion. And the period of dentition itself is very often an exciting cause, especially if the inflammatory condition of the child, at that time, be not properly attended to.

There is little doubt also, that particular constitutions of the air, which determine other epidemic diseases, as scarlatina, measles, hooping cough, &c. produce a tendency to hydrocephalic irritation. Why should not the membranes and vessels of the brain be under the influence of atmospheric causes, as well as those of the lungs, intestines, &c.? Itard, a French physician of eminence, distinctly asserts, that during two years when hydrocephalus was peculiarly prevalent, the one was remarkable for an epidemic scarlatina, the other for an ataxique fever among children.* Vieusseux observed this disease epidemic at Geneva.

Proximate Cause. Dr. Quin thought he had made a notable discovery in separating hydrocephalus acutus from *dropsy*, with which Whytt, Fothergill, and others had confounded it, and

* Dict. des Sciences Med. Vol. xxii. p. 223.

places it to the account of a morbid circulation or accumulation of blood in the vessels of the brain, sometimes rising into a state of inflammation, and terminating in effusion. Dr. Cheyne adopts a nearly similar theory, conceiving "that in this disease there is produced a venous congestion, in addition to, and probably arising from, the increased arterial action:—that the effusion of serous fluid arises from this venous congestion:—that this effusion has a tendency to counteract the baleful effects of the increased action, and to retard the fatal termination of the disease: of course, that the effusion into the ventricles is not the cause of the violent symptoms; and, that the increased arterial action, though perhaps varied, does not cease when the congestion and effusion have taken place."

The late Dr. Rush, of America, was one of the first who began to have more rational ideas respecting the nature and treatment of this disease, by directing the attention of practitioners to the vascular activity of the vessels of the brain, and to the advantage of blood-letting; and by the following remark, approached nearer to the facts than any doctrine which had been hitherto published. "No more," he says, "occurs in this disease than what takes place where hydrothorax follows inflammation of the lungs, or where serous effusions follow inflammation of the joints." *Inquiries*, vol. ii. p. 216.

Now, the researches *post mortem*, and the most successful modes of treatment, of modern physicians, have proved, almost to a demonstration, that the above observations will strictly apply to all acute dropsies, wherever situated; whether in the bags of the pleura, or among the convolutions of the intestines. The proximate cause, therefore, of hydrancephalus acutus, is the same as that of acute hydrothorax or ascites. As far as we can judge by symptoms and by dissections, we have every reason to believe that an IRRITATION, more or less approaching to INFLAMMATION, is determined on the arachnoid and pia matral tissues of the brain, producing, if not checked, a superabundant serous exhalation from their surfaces, in the same way as the inflammatory irritation of acute rheumatism, when determined on the synovial tissue of the knee-joint will, in a short time, fill the articular cavity with a

serous exudation. How far a diminished action of the absorbents may be concerned in this state, we cannot tell; but we know, with certainty, that an irritated or inflamed membrane will throw out a preternatural quantity of fluid. We entirely agree, therefore, in the following sentiments of the elder Dr. Parry, whose *Elements of Pathology*, we are sorry to see so little read or attended to by the profession, *in general*.

“Whether (says this excellent physician) inflammation exist in acute hydrocephalus, or not, all the previous and concomitant symptoms are those which show increased impetus; and thus, if the general principle which I have endeavoured to establish with regard to dropsies, be well founded, this disease, and the other symptoms attributed to increased determination of blood to the brain, throw mutual light on each other.” *Elements of Pathology*, &c. 352.

Symptomatology. In no disease is there a greater difficulty of laying down a general description of symptoms than in hydrocephalus, so proteiform are these symptoms themselves. We can only delineate the more prominent and constant features. In *idiopathic* hydrocephalus there is, for the most part, pain in the head, more or less acute, from the beginning, accompanied by a preternatural irritability in the organs or nerves of sense, especially of the eye and ear. Light annoys the child, and sounds which, in health, would produce little or no sensation, now cause him to start, or even cry out. When in bed, the child is constantly rolling about its head on the pillow, or putting its hands up towards its face, while slight agitation may occasionally be observed over the whole frame. A febrile state, especially towards the evening, may now be detected. The pain in the head frequently alternates with pain in the back of the neck, in the shoulders, arms, or pectoral muscles. The expression of countenance is perpetually changing, as is the rythm of the pulse, and the temperature of the body. The tongue is generally clean, or only slightly furred; the epigastrium tender, particularly when the headach is least complained of. There is marked depression of strength, except during the presence of convulsions; the functions of the digestive organs are impaired; the evacuations are yellow or

clayey, slimy, and foetid, and they speedily turn green on exposure to the air. The urine is highly coloured or turbid, and the sediment is of a white, glairy nature. The stomach is generally though not always irritable. The eye is much looked to in hydrocephalus; but, like the pulse in other diseases, it is very fallacious. In the stage of irritation it is morbidly sensible, and the pupil closely contracted; but as effusion takes place, the pupil widens, and the eye loses its lustre and expression, till at length strabismus itself ensues with convulsions of the whole body.

The stage of irritation or excitement is of various duration, from two or three days to as many weeks; and this slow or quick progress has led to the very useless division of the disease into different forms, according to the march or violence of the symptoms.

When effusion has once taken place, although the symptoms of inflammation and irritation experience a considerable mitigation, they do not entirely subside; and this circumstance renders it very difficult to ascertain the exact period of effusion, and consequently the time for changing our mode of treatment. When the fluid is extravasated to a certain extent, the symptoms of a compressed brain, as stupor, grinding of the teeth, convulsions, hemiplegia, projection of the eyes, squinting, blindness, injection of the conjunctiva, puffiness of the face, and, finally, apoplexy ensue, and terminate the scene.

Nothing, however, can be more irregular, as was before observed, than the progress and development of the foregoing symptoms. Very often the headach is not complained of till effusion has actually taken place. At other times, the most alarming symptoms will suddenly remit; and during this remission, the little patient will appear in almost its ordinary health, and that too when it is on the very verge of the fatal goal! The child's sensibility returns; the fever disappears; he recovers his usual liveliness; returns to his play; takes nourishment; and all danger seems to vanish. The parents, and even the medical attendant, deliver themselves up to hope and joy; but, all at once, the scene changes; the symptoms of effusion on the brain return with terrible rapi-

dity, and convulsions quickly terminate the life of the little patient.

These flattering remissions of the symptoms of hydrocephalus may sometimes be accounted for by evident changes in the balance of the circulation at the time, or by an increased determination to, or discharge from, other parts of the system. Thus they have been seen to subside on the breaking out of a profuse salivation from mercury; on the establishment of a great discharge from blistered surfaces, or the sudden supervention of œdema of the lower extremities. These, of course, relieve for a time the cerebral congestion, on which the symptoms depend, but do not always secure the organ from a subsequent attack.

In this disease, as in apoplexy, it is not absolutely necessary that there should be effusion from the vessels before death ensues. An epidemic hydrocephalus prevailed at Geneva in the year 1805, and terminated fatally from the second to the fourth day, without any very alarming symptoms. Frequently, on dissection, no water was effused, and death seemed to result, in such cases, from the violence of the cerebral irritation, or perhaps engorgement of the cerebral vessels. Upon the whole, there is the greatest uncertainty in every thing that respects hydrocephalus, except its two principal stages; the *first*, of cerebral irritation, the *second* of cerebral compression. The prominent characters of the *former* are headach, gastric irritability, agitation, delirium, fever more or less developed, morbid sensibility of the retina, pain or tension of the epigastrium, and of the cervical region. Those of the *latter* are dilatation of the pupil, strabismus, drowsiness or stupor, œdema of the face, paralysis, convulsions, insensibility.

Dr. Cook, after stating the various opinions of authors, relative to the diagnosis of hydrocephalus, concludes thus:

“From what I have read and seen, I am of opinion that when, in addition to fever, with vomiting and other symptoms of deranged stomach and bowels, we observe marks of uneasiness or pain in the head, drowsiness, morbid sensibility to light, a disposition to the horizontal posture, the face being turned from the light, and the hands placed round the head, there is strong reason to believe that water is actually effused in the brain. When the

pulse, from being frequent and regular, becomes slow and irregular, when violent pain in the head, with screaming, or a comatose state supervenes, when the pupil of the eye becomes either preternaturally dilated or contracted, with strabismus, the disease is still more manifest; but when the pulse, from having been slow, has become again very frequent, when the fever is very high, with flushings in the face, and inflamed eyes, when delirium in a great degree, or perfect stupor, with other symptoms, mentioned towards the end of Dr. Whytt's description, are present, I think there can be no doubt either of the nature or degree of the complaint." 433.

II. *Second Species, or Symptomatic Hydrocephalus.*

Such are the prominent traits of *idiopathic* hydrocephalus, a disease which undoubtedly exists, though it is much less common than the species which we are now about to describe. If it be less common, however, it is more fatal than the symptomatic species, and it is probably by confounding the two together, that we have so many discrepancies of opinion respecting the curability or incurability of the disease. Whytt, Fothergill, Watson, and some others, have laid down a most unfavourable prognosis; while Lettsom, Willan, Percival, Cheyne, Yeats, and many others, represent the disease, in its first stage, as greatly under the control of human art.

The *symptomatic* hydrocephalus occurs, perhaps, fifty times, where the idiopathic species occurs once; and it is from that species the great majority of our best descriptions of the disease are drawn, especially those of the *premonitory* symptoms. It cannot be expected, indeed, that an idiopathic cerebral irritation can long exist, without some degree of derangement in *all* the functions of the body, more especially those most commonly associated with the brain, as of the digestive organs; but if the case be carefully observed, it will be found that these are consecutive of the cerebral affection, and that they are, in general, slighter in degree than where they precede or give origin to the hydrocephalic irritation. It is the same with the hydrocephalic irritation itself; it is always more manageable when symptomatic than when idiopathic.

In childhood the digestive organs and the brain have a greater supply of blood, and a higher degree of excitability, than any other parts in the body. The *former*, till puberty, have an excessive function to perform, in building up the corporeal fabric; the brain, as the centre of the nervous system, to which all impressions are transmitted, is at that period endued with the most vivid sensibility. These two systems, then, are liable to numerous derangements, from the morbid stimulation of food and drink on the one, and moral irritation on the other. The two systems also act and react upon each other perpetually, and one cannot long continue disordered without drawing the other into a similar state.

A morbid state of the digestive organs so very *generally* propagates an irritation to the brain, that some intelligent physicians have been led into the opinion that hydrocephalus is *always* a secondary, or consecutive link in the chain of disease. Derangement of function or structure in the livers of children will, it is well known, very often induce all the symptoms, and all the fatal consequences of hydrocephalus. This was long ago insisted on by Professor Heineken of Bremen, and more lately by Doctors Cheyne and Curry; the latter, pushing the theory to extravagance, conceived that *all* cases of hydrocephalus were determined by a previous erythema or irritation of the liver. This circumstance only goes to prove, however, that such is *OFTEN* the case. More recently, Dr. Yeats of London, Dr. Wilson of Kelso, and Dr. Ayre of Hull, have brought forward powerful evidence of hydrocephalus being a consequence of disordered states of the chylepoietic viscera.

The following are Dr. Cheyne's reasons for considering hydrocephalus as very frequently dependent on abdominal derangements, as stated in his recent publication.

"1. Cases must have occurred to every physician, of children especially in the lower ranks of life, relieved by a short course of active purgative medicines, from a situation in which much of the expression, and many of the symptoms of hydrocephalus were combined—irregular fever, retching, headach, lethargy to

a great extent; symptoms which evidently arise from a disordered state of the abdominal viscera.

2. In many cases, previously to the appearances of hydrocephalus, the chylopoietic viscera have been disordered for many weeks. The appetite has been impaired, the bowels costive, the stools betraying disorder in the hepatic system; there has been all that want of alacrity, both of body and mind, so invariably the consequence of derangement of the biliary secretion; and in several children, previous to the existence of any morbid sensation, the first symptom of indisposition has been the loss of the healthy colour of the skin.

3. In children predisposed to hydrocephalus, while removing a vitiated biliary secretion and disordered state of the bowels, by a course of purgative medicines, in which mercury was generally an ingredient, the very same symptoms have been removed which had presented themselves in other children of the same family, when attacks of hydrocephalus, which actually proved fatal, were supposed to be established.

4. In children who had not any known tendency to disease, the symptoms, which are always found in the beginning of hydrocephalus, have often been removed by the same means.

5. In many cases it has been remarked, that children in the early stage of hydrocephalus, when the region of the liver was pressed upon, have complained much more than they did when the same pressure was applied to any other part of the abdomen.

6. In dissections after hydrocephalus, I have found the liver inflamed, adhering preternaturally to the peritoneum, enlarged, studded with tubercles, and otherwise differing from its sound state.

7. In one or two cases, alarming from their great resemblance to hydrocephalus, during recovery, the enlarged liver has been greatly reduced by mercurial purgatives."

In what way this morbid sympathetic influence is propagated we cannot, in the present state of our knowledge, say with certainty; but the *fact* is not the less true on that account. We may pretty safely aver, that it is through the nervous or vascular system, or both. Every man's experience has demonstrated the frequency of headach from irritation in the *primæ viæ*, especially the stomach. This must be principally through the medium of

the nerves. Now a strong degree of, or often repeated irritation, will, in a predisposed organ or part, soon induce inflammation; and this appears to be the case where hydrocephalus is determined by a morbid state of the digestive organs. A congested state of the liver, or a loaded condition of the intestines, may very easily be conceived to derange the balance of the circulation, and throw an undue proportion of the vital fluid on the brain. If a morbid irritation have been previously propagated to the encephalon from the digestive viscera, this increased afflux of blood will considerably aggravate the cerebral irritability; for it is as true that, *ubi fluxus ibi irritabilitas*, as the well known fact that *ubi irritatio, ibi fluxus*. Dr. Yeats has lately given some most interesting views of this subject, in the Appendix to his preceding work, and of which some account was presented in a former number of this Journal. Dr. Cheyne has also enlarged on this topic, in his recent valuable publication, to which we refer the reader.

Hydrocephalus Symptomaticus creeps on in an insidious manner; but an attentive observer will perceive the predominance of the *abdominal* derangements in the premonitory and early stages of the disease, and will often prevent the hydrocephalic symptoms entirely by removing the primary source of irritation.

In this form of the disease, the following, among many other premonitory symptoms, will commonly prevail; viz. languor, as if from fatigue; an unhealthy aspect of the countenance, evinced by transient paleness and collapse of the features; dark-coloured line under the eye, and dulness of that organ; the skin loses its soft pliable feel, and becomes harsh, with an increase of temperature. The appetite becomes capricious, with occasional thirst; the bowels torpid or irregular; the tongue white; the urine high-coloured, and sometimes sedimentous. All this time the vascular system remains tranquil, or but little affected. When the bowels are moved, unnatural secretions will, almost always, be perceived; the *fæces* will be sometimes too light in colour; sometimes greenish, clayey, or yeasty, with unpleasant fœtor.

“ Stools of the same nature with those which are generally passed in hydrocephalus, are seldom seen in any other disease. They resemble boiled laver, forming a dark green gelatinous

mass, with an oily-looking surface, and are of a sickly but not fœtid smell; they consist of flakes of inspissated bile, which gives them their colour, and of the mucus of the intestines. Diffused in water, they do not render it turbid, and scarcely change its colour; it seems as if the vitiated bile, by irritating the surface of the intestines, occasioned the copious discharge of mucus, to which the stools owe their consistence. When such stools once appear, they in general continue, till the disease is terminated by the death of the patient; and though I have known them made more copious, yet their character is seldom changed by a perseverance in the use of drastic purgatives. Unless the irritation in the bowels is allayed, the biliary secretion increased, and its nature changed, our efforts will be unavailing. Common cathartics increase the irritation of the bowels, which it is one of our great objects to diminish, and also carry the mercury out of the system before it has had time to make a sufficient impression upon it." Cheyne, p. 64.

If the abdomen be examined at this time, a puffiness or fulness of the epigastric region will be perceptible, with tenderness on pressure; the sleep is disturbed, and there will be some degree of emaciation, from the defective function of nutrition. All this time, there will be little more than uneasiness, rather than pain of the head, with some sense of external soreness. Children at this period, as Dr. Yeats has remarked, will often evince a precision of ideas and quickness of apprehension much beyond their years, and thus become interesting objects to their friends, rendering their loss more sensibly felt afterwards.

Upon this premonitory state of chylopoietic derangement, the symptoms of cerebral irritation more or less rapidly supervene, and then the train of phenomena characteristic of this accessory and dangerous affection, evince themselves in the manner already described under the head of *idiopathic hydrocephalus*, from which the symptomatic species cannot, in fact, be distinguished, after the disease is formed. Dr. Ayre, who has described this premonitory state of derangement in the digestive organs, under the name of marasmus, justly observes, that there is, in fact, often considerable difficulty, especially in infancy, to determine where

VOL. III.—3 E

the symptoms proper to the marasmus terminate, and those belonging to hydrocephalus internus begin.

But it is not derangement of the abdominal viscera alone that leads to cerebral irritation and effusion. Any febrile commotion in the system of a child, predisposed constitutionally to this insidious disease, will endanger the development of the complaint; for instance, the common remittent fever of infants, scarlatina, or any of the eruptive fevers. In such cases, the brain being the weakest organ, or that most disposed to irritation and inflammation, it suffers during the general excitement of the system, and hydrocephalus is the result.

The irritation of dentition is peculiarly liable to be propagated to the membranes of the brain, and there not only *simulate*, but produce the genuine symptoms of hydrocephalus. We say *simulate*; for we believe that, in many cases, the hydrocephalic phenomena are only *simulated* in dentition, because we have seen them all removed by the lancing of a gum, and a little aperient medicine, which would hardly have been the case, had the disease been actual hydrocephalus. In this way, many cures have been said to be effected, and remedies unmeritedly extolled, where the disease was counterfeited. Nevertheless, if this *simulated* affection be allowed to continue unchecked, in such young and irritable constitutions, it may, and no doubt often does, terminate in real hydrocephalus.

POST MORTEM RESEARCHES. It ought to be borne in mind that *irritation*, and even the primary stages of *inflammation*, are only lesions of the *vital properties* of parts; and that if these lesions occasion death [which, in such an organ as the brain, they frequently do] before they have altered the *structure*, or given rise to morbid *secretions*, they will *not* be cognizable on dissection. We are not therefore to conclude, that a disease was not hydrocephalus, because, on dissection, no effusion appears in the ventricles, or at the base of the brain. If, therefore, a child be suddenly cut off, while labouring under the usual symptoms of hydrocephalus, and dissection shews no adequate cause of death, we may fairly presume that the fatal event took place from *lesion of*

the vital properties of the brain, and before the structure was altered, or any water effused. The child probably dies in consequence of the vital energy of the brain being destroyed, either by too great irritation, or morbid vascular activity. The late Dr. Kirkland, of Ashby-de-la-Zouch, has some very good remarks on this subject, in his communications on apoplectic and paralytic affections.

In the majority of cases, however, both the causes and effects of the disease are sufficiently demonstrable in the brain after death. The principal, and the most common phenomena which present themselves on removing the skull-cap, are, a gorged state of the sinuses of the dura mater, and of the vessels spread over the surface of the brain. It is somewhat curious, that the vessels are sometimes distended with an æriform fluid, as was observed by Morgagni, Lieutaud, and Portal, in examining apoplectic bodies. The medullary mass itself, though sometimes soft and flabby, is, for the most part, rather firmer than natural.

Dr. Porter, of Bristol, in a most valuable paper on this subject, published in the third number of the *Medico-Chirurgical Journal*, has endeavoured to show, that genuine hydrocephalus acutus depends on a subacute inflammation of the *posterior* arteries of the encephalon, and that the symptoms during life distinguish, in many cases, this state from meningeal inflammation constituting true phrenitis. This ingenious and able physician, however, acknowledges that the two diseases "often glide imperceptibly into each other, as the inflammatory condition of the encephalon becomes more general." His conclusions are strongly supported by the dissections which he has made. In these dissections, was seen an abundance of gelatinous matter, and of coagulable lymph, covering the pons varolii and adjacent parts—unequivocal products of an inflammatory condition. In fourteen brains of patients dying of this disease, he did not, in one instance, find this appearance wanting.*

Dr. Cheyne remarks—"upon dissection, we generally find within the cranium, the veins, particularly those of the membranes on the surface of the brain, and lining of the ventricles, gorged with blood. Sometimes considerable adhesion between, and

* *Med. Chir. Journ.* No. 3. p. 289.

thickening of the membranes, and minute and florid vessels upon the pia matter." 12. Dr. Quin, after stating the vascular turgescence of the brains on dissection, goes on to say, that "in most of them a degree of inflammation had taken place, as appeared at the time of dissection, either by preternatural adhesions of the membranes, or by a partial opacity, and increased thickness of them, together with patches of inflammatory crust, very similar to those which are found on the abdominal viscera of persons whose death has been the consequence of *enteritis*, or on the lungs and pleura of those who have sunk under pulmonic inflammation."

From the modern pathological researches of M. Laennec, and several other continental physicians, it appears that, on minute examination of the brains of hydrocephalic patients, small tubercular granulations have been found dispersed through the cerebral and cerebellic mass, in the thalami nervorum opticorum, and even in the thickened portions of the meninges themselves. This interesting fact has been brought to light by the patient and laborious pathological investigations of our continental brethren, whose researches, *post mortem*, have been greatly facilitated by the peculiar economy of their public institutions, and by the want of that morbid antipathy towards dissection, which cramps both public and private practitioners in this country. This morbid organization, discovered by glasses, and by minute anatomy, may explain much of the mystery which has hitherto hung over hereditary and local predisposition in hydrocephalic subjects, as also the connection between this disease and the scrofulous diathesis. The morbid organization described by Laennec and others, cannot justly be looked upon as the *effect*, so much as the *cause* of the hydrocephalic irritation, inflammation, and effusion.*

The most constant of all *post mortem* appearances is the effusion of water, sometimes into the ventricles, and sometimes about the base of the skull, and sometimes between the dura and pia mater. This effusion is looked upon, by the best pathologists of

* See what Dr. Porter says on this subject, at page 291 of the third number of this Journal. See also a dissection of Hydrocephalus, in the 4th vol. of the Medico-Chirurgical Journal, p. 427, where these scrofulous disorganizations were seen.

the present day, as the *effect* of the stage of excitement, irritation, or inflammation; and as Nature appears to pour forth this fluid on the surface of various other tissues, for the present relief of a congested, inflamed, or irritated state of the parts, so it is considered, by men of great discernment, that the hydrocephalic effusion, though *ultimately* fatal in its consequences, gives a *temporary* relief to the general excitement, and the local inflammation. This idea is strongly supported by the mitigation of symptoms, commonly accompanying the effusion.

The fluid effused has this peculiarity, that it is not coaguable by heat, acids, or alcohol; a fact which was noticed by Fabricius Hildanus, and confirmed by Watson, Lecat, Mathey, Vieusseux, Haldat, and others. Haldat found on evaporating the fluid effused in acute hydrocephalus, a brown residuum, consisting of muriate of soda, $96\frac{1}{2}$ parts out of 100; the other $3\frac{1}{2}$ parts consisted of water, albumen, mucus, gelatin, and phosphate of soda. It appears, therefore, that this fluid does not agree, in its chemical properties, with the fluids of other dropsies.

With the following observation on this subject, we were lately favoured by a distinguished physician of this metropolis, who has long directed his attention, with great success, to an investigation of the disease under review.

“The different effects produced by chemical tests in the water of hydrocephalus, and in the water of other dropsical effusions, I found verified in the *same individual*. The fluids were taken from an elderly gentleman, who had died, after long suffering, from diseased bladder and prostate. He had a hydrocele of one testicle; and water was also collected from the ventricles of the brain. The water from the *former* produced an immediate copious white curdy precipitate, by the addition of nitric acid. It was also rendered milky, and opaque by heat; and when raised to the boiling point, it became thick, like jelly; while, with the fluid from the ventricles of the brain, no alteration was produced by any degree of heat, nor by addition of nitric acid, whether made to it when it was cold or hot.”

In the investigations, *post mortem*, of hydrocephalic patients, it has been too much the custom to limit the examination to the

head, as the apparent, or at least, the supposed *fons et origo* of the disease. The spinal marrow, an important appendage to the brain, has been greatly neglected. Yet there is every reason to believe, from the symptoms during life, and the state of the medulla oblongata, on dissection, that the upper part of the spinal brain suffers in common with the inferior part of the cranial. In all Dr. Porter's dissections, the *tuber annulare* was involved in a mass of coagulable lymph, and some fluid escaped on dividing the medulla spinalis in its vertebral cell. It is not credible that the inflammatory action and its consequences should stop at a determined point on a continuous membrane; and the deranged functions of organs and parts supplied with nerves from the spine, afford presumptive proof that the spinal suffers in common with the cranial brain.

The abdominal lesions too, have been very much overlooked; for granting even that they were *consequential* of the cerebral affection, they are often destructive of life. Dr. Cheyne observes as follows:—"In the abdomen I have found the intestines inflamed, and *constricted from spasm*,* and the surface of the liver of a bright red colour, abounding in minute vessels, and sometimes extensively adhering to the peritoneum. In several of the dissections I have found the surface of the liver studded with small white tubercles, not larger than a grain of mustard. The glands of the mesentery are often diseased." A little farther on Dr. Cheyne remarks, "upon dissection of hydrocephalic children, I have found in the liver, the remains of great inflammatory action, and also proofs that undue irritation had existed in the alimentary canal." 12. *Essay I.*

Mr. Abernethy states his having examined three children who died of inflammation and effusion in the brain, "in all of which cases the liver was greatly diseased, and the bowels also exhibited a diseased appearance." "I have also, (says this distinguished observer,) examined a child who was supposed to have died of hydrocephalus, accompanied by a great disorder of the stomach

* This constriction from spasm, indicates a pressure on the origins of the nerves supplying the constricted portions of intestine. Rev.

and bowels. In this case, the bowels were inflamed, the liver sound, and the brain perfectly healthy in appearance." *Surgical Observations, Part Second.*

In nine dissections of hydrocephalus out of eleven, which Mr. A. T. Thompson made, there were evident marks of inflammation in the liver. In one of the remaining dissections, there was intus-susception of the jejunum; in the eleventh, inflammation of the colon. The same gentleman asserts, that in a majority of the cases of the disease, the organs of digestion are in fault, before the head appears in any degree affected. And he adds that, "in every opportunity which he had of observing the earliest approaches of hydrocephalus, the bowels have first become irregular; the stomach acescent, and the stools, whether procured by medicine or not, have been foetid or clay-coloured, displaying defective action of the liver, and an imperfect formation of the bile."*

Dr. Boyton, many years ago, also, came to the conclusion, that hydrocephalus originated in the abdominal viscera, from finding that cathartics were the principal means of cure. *Cheyne's Second Essay, p. 50.*

Many instances might be cited, to prove that disease in almost any organ of the abdomen or thorax may give a development to the hydrocephalic irritation. In the third volume of the *Medico-Chirurgical Journal*, (Monthly Series) there is an interesting case of this disease, where, on dissection, the right lung was found studded with scrofulous tubercles, and six ounces of effusion in that side of the thorax, with marks of inflammation on the pleura. Here water was found in the ventricles of the brain, but no affection of the coverings.

In conclusion, after maturely and candidly examining the evidence and general sentiments of the profession, it appears that, in a great majority of instances, dissection has shown *structural* derangements of other and distant parts accompanying hydrocephalus; and that in an equally great proportion of cases, the symptoms, during life, authorized the conclusion that *functional* derangements of these distant parts preceded the irritation, inflam-

* *Edinburgh Medical and Surgical Journal*, Vol. ii. p. 482.

mation, and effusion in the head:—in other words, that hydrocephalus *symptomaticus* is infinitely more common than hydrocephalus *idiopathicus*.

Under these impressions, we conceive that the medical profession is deeply indebted to Dr. Yeats for the steady and impressive manner in which he has kept the attention of medical practitioners directed to this most interesting point of pathology; and sincerely do we hope that this learned and enlightened physician will prosecute, with unremitted ardour, the track of investigation in which he has already distinguished himself.

TREATMENT OF HYDROCEPHALUS.

Theory has always had a great influence on practice, and often produced incalculable mischief. Pathology is now the prime mover in therapeutics; but comparatively safe as is this guide, it may, and does occasionally, lead us astray. The circumstance of finding water effused in the ventricles of a hydrocephalic patient has led to very bad practice. Dr. Smyth, in a late work on this disease, appears to view the *effusion* as the great cause of the symptoms, and consequently that its *absorption* is the grand indication of cure. Here then is an instance where morbid anatomy has led to an erroneous practice. In ascending a link higher and viewing the pathology of the disease as *inflammation*, we shall often go wrong; for something precedes even this. In short, unless we keep steadily in view the relation between the nervous and vascular systems—between the disorders of *function* in the early part of the disease, and the disorganizations of *structure* in the end, we shall never obtain a comprehensive and enlightened pathology, or a rational and successful practice, in this or in any other disease. The *methodus medendi*, in hydrocephalus, will always be vacillating too, while men pertinaciously adhere to confined notions, and partial views of the etiology of the complaint. We have endeavoured to show that hydrocephalus is not always, nor even generally, an idiopathic disease of the brain or its coverings; and that it does not arise *exclusively* from derangement of this or that organ, as some biassed practitioners believe,

but *occasionally* from derangement of any one of them. The enlightened and unprejudiced observer, then, will examine, with all possible accuracy and minuteness, into the history of each individual case—into the previous and present state of each organ and function, and thereby endeavour to trace the morbid chain, link by link, from the first cause to the ultimate effect.

The great danger of viewing hydrocephalus as either *always* idiopathic or *always* symptomatic, consists in this, that the party so viewing it, though right in his opinion, will be exceedingly liable to err in his practice. Thus, suppose a man who believes hydrocephalus to be an idiopathic affection of the brain, and the disordered functions of other organs at the time, merely symptomatic—and supposing that this is actually the case, which is the most favourable supposition, such practitioner will direct his remedies almost exclusively to the seat of the idiopathic affection, and not attach nearly sufficient importance to the sympathetic or consecutive derangements, though these last are, in fact, to be treated with *equal* care, as if they were the primary links of the chain. The same observation applies to the man who views hydrocephalus as symptomatic of abdominal disorder. He treats the cerebral affection as a secondary one, in point of consequence, though it deserves *equal* attention, as the visceral derangement at a distance. The golden rule, then, and that which is most easy of application, is, to direct the energy of our measures to the head or abdomen, in proportion to the urgency of the symptoms, in this or that region, always remembering, however, that in a great majority of cases, the functional derangement of the abdominal organs is the primary link in the morbid chain: and, that whether primary or secondary, its removal is the *sine qua non* of safety.

When we are called to a child labouring under functional disorder of the digestive organs, as evinced by unnatural secretions from the bowels, and some degree of morbid *irritability* in the whole system, but with no particular affection, apparently, of the sensorium, we should examine carefully into the history of the complaint—into the constitution, not only of the patient, but of the family—and into the state of the abdominal organs generally,

by pressure on every point of the belly. If we find the abdomen tense, it is evident that the intestines are distended either with flatus or fecal accumulations; and this distention deranges the circulation of blood in the ventral viscera, besides keeping up a constant irritation in that region. This irritation is liable to be propagated to the brain, to which more blood is sent, in proportion to the difficulty of its transmission through the abdominal organs. The first indication here is self-evident. It is to clear the whole line of the intestines by such purgatives as may promote all the secretions; intestinal, pancreatic, and biliary. Two or three grains of calomel, followed in an hour or two by a solution of some neutral purging salt, should be administered, and repeated, if necessary, till the intestines are freely evacuated. The evacuations themselves will enable us to judge of the state of the bowels. Their clayey, or variegated appearance, and offensive odour, will point out the necessity of *correcting* this morbid state of the secretions, as well as of evacuating them. But it frequently happens, that in every stage of hydrocephalus there is a most obstinate torpor of the intestinal canal, which torpor is, in all probability, owing to the state of the brain, and of the origins of the nerves supplying the abdominal viscera. This circumstance alone, even where the head is apparently unaffected, is a sufficient indication for leeching the temples, and applying stimulating frictions along the spine. By these means, and by the mercurial purgatives, we act both on the origins and extremities of the nerves in question, and thereby greatly promote the abdominal functions. But purgatives, local blood-letting, and spinal frictions, though they may, in many cases, succeed, are not to be exclusively trusted to. Continued purging by drastic medicines may even do harm by keeping up too much irritation in the digestive organs. In the intervals of purging, our aim should be to restore the morbid secretions to a more healthy condition, and this will be best effected by small doses of calomel, joined with antimonial powder, and where much irritability of the system prevails, even adding a small proportion of opium, in the form of the pulvis cretæ comp. c. opio. Hypothetical objections are formed against this medicine, under the idea that hydro-

cephalus was common inflammation of the brain. But it ought to be remembered that *irritation* in the system, and in that organ, often forms the very basis of the complaint, and, at all times, aggravates the inflammatory condition of the cerebral vessels. Half a grain of calomel, and a quarter of a grain of antimonial powder, with or without the opiate, ought to be given every three hours; and this small dose will be found to answer better than larger ones. Every day the bowels ought to be cleared by a saline or oily laxative, after the operation of which, the calomel should be again continued without interruption.

Where there is fulness or tenderness of the region of the liver, leeches should be applied to that region, in numbers proportioned to the degree of congestion there; and the tepid bath every evening will greatly assist the operation of the other remedies, especially if impregnated slightly with the nitro-muriatic acid.

All this is presupposing that the head has, as yet, evinced no particular symptoms of irritation or inflammation. The moment the cerebral disorder becomes manifest, or threatens in the least, then the most active means must be taken, in addition to those enumerated, to save the structure of the brain, and prevent effusion. These means are local or general blood-letting; cold to the scalp; blisters to the nape of the neck and spine; frictions; elevation of the head; quietude; darkness.

The jugular vein or temporal artery may be opened; but in general, leeches, in sufficient number, not less than from eight to twelve, should be applied to the temples, nape of the neck, and upper portion of the spine; immediately after which, a long and narrow blister should be applied from the occipital foramen to the upper dorsal vertebra, and a discharge kept up by savine cerate or unguentum lyttæ. The head must be shaved, and clothes wet with vinegar, water, and a little spirit, should keep up a constant abstraction of heat from the scalp. This process is never to be neglected, though it too often is so, while the stage of irritation or excitement lasts.

During this time, the mercurial plan, to correct the secretions and keep the intestinal canal clear, is not to be interrupted; and as the liver is very frequently in fault, *mercurial liniment* should

be well rubbed along the spine and region of the liver, every time the child comes out of the tepid bath, which should not come higher than the pit of the stomach. The mercurial liniment is one of the best stimulants that can be applied to the spine, as it answers a twofold purpose, that of exciting the energy of the nerves arising from the medulla spinalis; and of entering the system, so as to act upon the hepatic and other secretions, and thereby relieve the head.

“I have myself (says Dr. Cook, in his learned researches on this subject,) witnessed the good effects of mercury used both externally and internally, in several cases of what I supposed to be water in the brain; of these, two were marked by the symptoms of confirmed hydrocephalus. On the whole, the general opinion appears to be in favour of the use of mercury. Indeed, Dr. Abercrombie seems only to deny its specific influence, and to object to the indiscriminate employment of it. The instances of recovery from this dangerous complaint, under the management of Dobson, Perceval, and many others, as well as those which have fallen under my observations, may, I think, be fairly adduced in support of the practice in question; and I believe few physicians of the present day would think themselves justified in omitting to employ mercury in attempting the cure of this complaint.” 466.

The effects of digitalis in the stage of excitement are very doubtful, and after effusion has taken place, still more so. It is a medicine, however, which is pretty generally employed, on the principle of arresting the velocity of the circulation, by checking the inordinate action of the heart. Dr. Cheyne informs us, that he begins with a dose of eight or ten drops of the saturated tincture, every six hours, increasing it gradually by two or three drops at a time, till constitutional symptoms supervene.

It ought ever to be borne in mind, that the symptoms of actual *effusion*, in hydrocephalus, are sometimes very equivocal; and may be closely imitated by turgescence of the cerebral vessels. This, coupled with the almost certain fatality of hydrocephalic effusion, should induce us not to desist from the depleting plan, so soon as the symptoms of effusion manifest themselves. It is moreover ascertained by dissection, that the inflammatory condition of the

cerebral vessels does not always, nor even generally, subside when effusion takes place. We have opened many heads, where water was effused, and yet inflammation was going forward. Nor is this peculiar to the head; the same happens in the chest and other parts. We this day [16th of April, 1819,] opened a body, where the immediate cause of death was effusion in the lungs, cavity of the pleura, and into the pericardium; yet the internal surface of the pericardium, and the exterior surface of the heart were in the highest possible state of inflammation, notwithstanding the effusion of serum and coagulable lymph in all directions.

It is evident that in hydrocephalus, as well as in any other disease, where effusion is going on from irritation, turgescence, or inflammation, if we arrest the depleting plan, at such a juncture, we only give increased power to the disease.

Whether, when effusion is complete, and the inflammation reduced, the effused fluid is ever absorbed from the ventricles of the brain, is exceedingly problematical. We should suppose that slight effusions may be absorbed, especially from between the coverings of the brain. Because there are few or no absorbent vessels traceable in the cerebral mass, it does not follow that no absorption can take place, since recent experiments prove that the VEINS can perform that office.

The vapour bath has lately been recommended by some continental physicians; and a few cases are published, where this measure is said to have proved successful, even where effusion had actually taken place. A case is related in the Medical Commentaries for 1782, where the vapour bath, as is attested by Doctor William Hunter, restored a child, after all the symptoms of effusion had appeared in the worst degree.

We are disposed to hope, that in this short *Exposé* of the therapeutical means to be employed in hydrocephalus acutus, will be found most of those that are really useful, and easily applicable to practice. We have been more anxious to investigate the nature and pathology of the disease, than to enumerate a long list of remedies, or to lay down a systematic code of treatment. The latter must vary in almost every individual case; and the man who entertains the most comprehensive views of the pathology,

will always be most capable of adapting the remedial means to the end or objects desired.

It will also have been observed, that the foregoing paper has not been drawn from any particular source—nullius addictus jurare in verbo magistri—but that we have ranged freely over the field of medical literature, while no inconsiderable share of it is the result of personal observation and reflection. Yet we cannot conclude, without taking some exclusive notice of Dr. Cheyne's work, which did not come to hand till the greater part of this article had been prepared for the press, although many of his statements are quoted from the first edition of his *Essays*. The present republication contains a great mass of important observations and highly interesting cases, (some of them new,) which eminently entitle it to the study of the Profession. From the second essay of this very able and experienced author, we shall introduce to our readers a few extracts relative to the treatment of the disease under consideration.

“The state of the hypochondria, the nature of the stools, and the other excretions, the appearances of the tongue, and the smell of the breath, ought to be examined with care. If the patient wince when the right hypochondrium is pressed, leeches ought to be applied to it, or the margin of the ribs may be cupped and scarified; if there be much pyrexia with headach, blood must be drawn from a vein, or from the temporal artery. Then cathartics are to be given, to promote, and, if necessary, to alter the secretions; generally calomel, with small doses of some common purgative of an active kind, as rhubarb, jalap, or scammony, with aloes. If there be a sickly smell of the breath, and fulness and uneasiness at the pit of the stomach, an irritation of the mucous membrane of the intestines is denoted, which is sometimes relieved by mild antimonials; these consequently are to be added to the cathartics; squill may also be exhibited with the same view, more especially when the urine is deficient. If the stools be dark green and glairy, most probably the common cathartics will have little effect; indeed, we cannot expect that they will change the nature or appearance of the secretion, which issues from an organ over which they have little control.

“Even calomel, the medicine from which most might be ex-

pected, is sometimes inert as a purgative, and has no influence over the system as a mercurial; and this seems to arise from want of suitable preparation. In the cases in which hydrocephalus seems most remarkably to have its source in a disorder of the abdominal viscera, and in which the cure is to be effected by exciting these organs to free secretion, we are generally unable, after the first day or two, to effect that purpose by direct means.

"It is a well known law in pathology, that if a gland be excited beyond a certain point, it is no longer able to perform its discerning function; and when so circumstanced, a stimulus applied to it, instead of restoring secretion, often increases the vascular excitement upon which its interruption depends. In hydrocephalus, the biliary secretion is generally languid as well as vitiated; and the presumption is strong, that this condition of the bile depends upon the general vascular excitement of the liver. If the viscera of the abdomen, and particularly the liver, are in a high state of irritation, this irritation ought to be allayed before the stimulus which increases their secretions can be employed with advantage, or even with safety. The true practice is, in the first place, to reduce arterial action by venesection, or by topical bleeding and blistering, and then to restore the discerning function of the viscera by means of calomel and other cathartics.

"In high degrees of vascular excitement of the liver, as, for instance, in hepatitis, mercury, which is the specific remedy, is beneficial or injurious, according to the condition of the liver when it is administered. It is injurious till, as Dr. Johnson says, 'the inflammatory congestion in the liver is relieved by free blood-letting.*' I formerly quoted a passage of a similar import from Dr. Macgregor's *Medical Sketches*. The power of blood-letting in forwarding the operation of mercury, might be shewn by a number of cases and observations. In the endemic hepatitis, described by Mr. Chisholme,† the practice pursued, with remarkable benefit, was to let blood freely in the first instance. 'After the third bleeding,' he remarks, 'we gave from two to seven grains of calomel, with from one-fourth of a grain to a whole grain of opium, three times a day. This practice,

* *Influence of Tropical Climates, &c.* p. 282.

† 1st Volume, 2d Decade, *Duncan's Medical Commentaries*.

continued for two days, brought on a copious salivation; when this was effected, we considered the patient out of all danger, and it was astonishing how readily cases of the most dangerous tendency were cured by this method in a few days."

When hydrocephalus appears to be a secondary disease, our author recommends us to begin the cure by attempting "to relieve the irritation of the abdominal viscera, which is never fully accomplished until their healthy secretions are restored by cathartics." "The cathartics applicable to hydrocephalus are those which increase the secretions. We may remove *fæces* from the alimentary canal, by quickening the peristaltic movements, with but little benefit to the patient. Our object is to change the action of the *secerning* vessels, and more especially encourage and improve the biliary secretion; but in so doing we must avoid irritating the mucous membrane of the intestines."

As the milder antimonials have the effect of increasing the secretions of the liver and *primæ viæ*, Dr. Cheyne recommends them in combination with mercurials.

Our author has added a series of cases, in which, after depletion, opium, in conjunction with mercurials and antimonials, was given with the best effects. He observes, that in diseases attended with increased activity of the circulation, a very moderate dose of opium will be found sufficient. The same is applicable to affections of the bowels, where opium, in excess, might interfere too much with their peristaltic movements. "If it be wished (says Dr. Cheyne) to place the constitution suddenly under the influence of mercury, I know but of two means, apparently opposite, namely, blood-letting and opium." This we can confirm from pretty extensive observation.

Dr. Cheyne introduces some euthanasial hints relative to the effects of blood-letting in mitigating sufferings, where we are without a prospect of cure. He has long been convinced that patients who die of organic diseases of the brain, struggle and suffer much more when they are not bled than they would do, were venesection performed. It is well known, indeed, to our West India practitioners, that blood-letting smoothes the way to the grave, without quickening the pace of the victim of yellow fever. "The convulsions (says Dr. Cheyne) which attend

hydrocephalus may be removed by affusion, or rather aspersion of the face and neck with cold water;" which he thinks is preferable to the warmbath, which is more operose, and less effectual. He recommends "the injection of a large glyster, made by dissolving phosphate of soda or sulphate of magnesia in broth, after which children in hydrocephalus generally lie easy for a couple of hours."

We are sorry that the great length to which this article has run, prevents our introducing any of the great mass of highly valuable cases appended to this volume; but we are in hopes that most practitioners will peruse the work itself, which is in a compendious form and small type, worthy of imitation in the present period of general distress.

To Dr. Yeats's Work and Appendix, together with Dr. Cooke's erudite Compilation, we would also wish to draw the attention of our brethren; and if they derive from their perusal as much pleasure and information as we have done, they will be amply repaid for their labour.

The following article is an extract from a very interesting and able "Review of Practical Medicine," published in the London Medico-Chirurgical Journal for April, 1820. Want of room obliges us to select only such parts of it, as appear to us more particularly interesting, to our medical brethren of the United States.

EDITOR.

A REVIEW OF PRACTICAL MEDICINE;

SELECTED AND ARRANGED, WITH COMMENTARIES.

Paucis libris immorari et innutriri oportet, si velis aliquid trahere, quod in animo fideliter hæreat. SENECA.

*Duo vitia vitanda sunt in cognitionis et scientiæ studio. ***** Alterum est vitium, quod quidam nimis magnam operam conferunt in res obscuras atque difficiles, easdemque non necessarias. CICERO.*

§ I. HEAD.

I. *Cephalea*.* A young woman, ætat. 24, had been afflicted with constant beating pain in the left cheek and upper jaw, along

* By ROBERT LISTON, Esq. &c. &c. Ed. Journal, 62.

the alveolar process, stretching to the throat, "and, indeed, involving the whole head." The pain was without respite, and so violent, that she constantly supported her head, unable to use the slightest exertion, and sleeping only when completely exhausted. "Of course, all kinds of remedies had been tried, external and internal." This is a very vague expression, especially when Mr. Liston solicits suggestions respecting the treatment which has hitherto been unsuccessful. Who can prescribe, after "all kinds of remedies" have been used in vain. But the fact is, that all kinds of remedies had *not* been tried, for Mr. Liston himself put in practice a new one; namely the ligature of the carotid artery of that side. He was naturally enough led to this experiment, by finding that pressure with the thumb upon that vessel quickly mitigated all her sufferings. For some time after the operation, the patient was much relieved; but since then, the pains have returned with violence.

We have often had occasion to observe the erroneous importance which is attached to the *vis a tergo*, or, as Dr. Parry expresses it, the momentum of the circulation. When a tissue, or structure of any kind, is in a state of irritation, inflammation, distention, or organic lesion, every impulse of blood from the heart, in general, augments the pain. But where numerous channels lead to the seat of lesion, it is in vain to look for permanent relief from the obliteration of any one. The vascular distention is again reproduced through other circuits. The object is clearly, according to our apprehension, to reduce the whole circulating mass; to keep it in a state of reduction proportioned to the local lesion; and, during that time, to endeavour to restore the healthy state of the part itself. This must be done by local and general blood-letting, rigid abstinence, constant action of the bowels, and quietude. In short, not only should the balance of the circulation, but that of the nervous excitement, be diverted, if possible, to another quarter than the head; especially to the intestinal canal—*qua data porta*. We know very well that it requires great confidence in the physician to have these measures rigidly, and for a sufficient time, put in execution; but this much we can assure Mr. Liston, that by an inflexible and persevering course of the fore-

going measures, we have overcome diseases, after "all kinds of remedies had been tried," with no kind of energy or effect.

II. *Ophthalmia*.* The continental physicians have been much more attentive to those modifications of inflammation resulting from the nature of the texture invaded, or the constitutional disease of which the inflammation is the local demonstration, than we have been. The attention of British pathologists, however, is now directed to this investigation; and we have no doubt of a beneficial result.

1. *Description*. Mr. Wardrop observes, that in the *Rheumatic Ophthalmia*, the red colour of the *albuginea* is not the bright crimson accompanying inflamed *cornea*; nor is the redness confined to one spot, as in pustular ophthalmia. Neither is there the puriform discharge attending inflamed conjunctiva. The *albuginea* acquires a brick-red tinge, or an admixture of yellow with crimson red. The blood-vessels are generally equally numerous over the whole white of the eye, passing forward in nearly straight lines from the posterior part of the eye-ball, and advancing close to the *cornea*; but neither passing over it, nor leaving the pale circle around it, which is so striking when either the choroid coat or iris is inflamed. As the disease advances, the *cornea* becomes dull and turbid, sometimes with a general cloudiness, more opaque in the centre than circumference. If minutely examined, some abrasions of the *cornea* and conjunctiva will be detected. In the beginning there is a dryness of the eye, succeeded, sooner or later, by a very copious lachrymal secretion. The eyelids in this disease are but slightly swelled, and their vascularity little increased. The chief seat of pain, at the commencement, is in the head, sometimes affecting the eye-ball itself, or extending to the temple, cheek-bone, teeth, or lower jaw. Occasionally there is precise hemicrania, of a dull agonizing, rather than an acute kind, and coming on at times in severe paroxysms. Sometimes the pain is excited by merely touching the scalp; it is usually remittent, com-

* An account of the Rheumatic Inflammation of the Eye, with observations on the treatment of the diseases. By JAMES WARDROP, Esq. F. R. S. E.—*Medico-Chirurgical Transactions*, vol. x.

ing on at four, six, or eight o'clock in the evening, continuing all night, being most severe at midnight, and becoming abated towards morning. The eye is not very morbidly sensible to light in rheumatic ophthalmia.

Our author considers the seat of this affection to be the sclerotic coat chiefly, which belongs to the fibrous tissues, the favourite domiciles of rheumatism.

To the local symptoms there is generally superadded pyrexia, with much functional disorder of the *primæ viæ*, and vitiation of the secretions and excretions. The progress and severity of the disease are very various; the attack being sometimes slight and transitory, leaving no permanent injury of the eye. At others, long and severe, ultimately destroying the visual organ, by ulceration of the cornea bursting forth of the aqueous humour, or puriform secretion in the posterior chamber of the eye.

In respect to etiology, rheumatic ophthalmia may, in many instances be traced to sudden atmospherical transitions; is most frequent in the spring months, and often follows the operations for cataract, especially in patients of rheumatic tendency. It usually affects but one eye, sometimes both, the inflammation being seldom so severe in the second eye as in the one first affected.

The rheumatic resembles the syphilitic more than any other ophthalmia, and Mr. Wardrop has frequently seen them confounded; yet he thinks they may be distinguished from one another by the appearances of the inflammations, the progress of the diseases; but above all, by the constitutional symptoms which accompany them.

“In the rheumatic inflammation, says Mr. W. it has already been noticed that the proper vessels of the sclerotic coat are enlarged, which is the cause of the redness being generally diffused over the whole albuginea; whereas in the syphilitic inflammation it is the anterior ciliary arteries passing along the sclerotica on their way to the iris, which are chiefly affected; and thus [hence] the pale ring which is always observed between the cornea and enlarged vessels in that disease.” 10.

Those too who have the syphilitic inflammation in the eye have

always, our author avers, the constitutional symptoms of syphilis, the history of which can be distinctly traced.

2. *Treatment.* In this species of ophthalmia, Mr. W. has found evacuation of the aqueous humour advantageous; especially where proper means had been neglected in the beginning; where there was attendant pain; where the cornea had become dim and clouded; and where the vision was impaired. In all such cases, the good effects of the operation were instantaneous; after which, he found no other applications necessary, than fomentations to the parts around the eye, and, where the eye itself remained irritable, some vinous tincture of opium.

The digestive organs, in this complaint, as indeed in most others, require great attention. Mr. W. has seen much relief obtained from the exhibition of an emetic, composed of vinum ipecacuanhæ one ounce, and a drachm of the liq. antimon. tart. After the emetic the bowels should be completely evacuated; "and a couple of grains of calomel combined with a few of rhubarb may be two or three times given; also saline, or other purgatives, if necessary, with a view to act on the biliary organs, as well as empty the alimentary canal." 22.

When this inflammation has succeeded a sudden chill, the functions of the skin should be restored by the judicious employment of sudorific medicines. "A couple of grains of antimonial powder given singly, or combined with opium, every four or six hours, is an excellent remedy," often allaying the evening paroxysm, when exhibited a little before the accession. Our author has seen little advantage derived from local or general bleeding, unless there was tendency to plethora, with a very full and hard pulse, when one or other of the evacuations would be proper. But, in general, "the little relief afforded by bleeding in this disease, may be regarded as one of its diagnostic characters." Fomentations and blisters, repeatedly applied behind the ears, or to the nape of the neck, are useful auxiliaries. "The vinous tincture of opium is the only local application which he has ever observed to be decidedly beneficial; but its use should always be deferred till the latter period of the inflammation, when all febrile symptoms are subdued." A small quantity of this fluid may be put

within the eye-lids once or twice a day, with a common camel's hair pencil, and its use persisted in while it affords relief.

If, after the *primæ viæ* have been well evacuated, the tongue still remain very white, with quickened pulse, Mr. W. has seen decided benefit from *cinchona*, singly or combined with the mineral acids. It should only be given in small doses, generally from five to eight grains in a little warm water every two hours.

Mr. Wardrop was led to this treatment from the remarkable effects which he had observed, in the cure of rheumatism of the joints, from *cinchona*. Mr. W. appears to think bark as complete a specific in this disease, as in ague; but we fear that the generality of the profession have not been so fortunate in finding a specific for rheumatism. The paper before us is very creditable to Mr. Wardrop's well-known talent for acute observation, and felicity as well as facility of description.

While on the subject of ocular inflammation, we may here notice an observation of Mr. Guthrie, in his recent publication on artificial pupil.* It is well known that in idiopathic iritis, blood-letting and mercury have been found the paramount measures of cure. Mr. Guthrie, in the work above mentioned, informs the profession, that, in the iritis which occasionally supervenes on the operation for artificial pupil, mercury is equally efficacious as in the idiopathic, or, as some have supposed, the syphilitic forms of the disease. "The great utility of mercury in inflammation of the iris resulting from wounds, has not, I believe, been either noticed by authors, or generally understood. It is not, however, more valuable in the one case (idiopathic) than in the other, (the traumatic), and the use of it should never be neglected, when bleeding is insufficient for the suppression of the inflammation, which will, in all probability, prove destructive to vision." *Treatise*, p. 157.

* A Treatise on the operations for the formation of Artificial Pupil, in which the morbid states of the eye requiring them are considered, &c. &c. By G. J. GUTHRIE, &c. &c. &c. with Plates. Octavo, pp. 210, London, 1820.

III. *Ophthalmia Periodica*.^{*} We should conceive that the patient in this case was also the physician.—J. B. ætat. 46, spare, and delicate habit, yet active, and without any hereditary disposition to disease, but subject to stomach complaints, becomes affected, in the month of June, every year, with a greater or less degree of the following symptoms: A sensation of heat and fulness in the eyes, with, at first, a slight degree of redness, and discharge of tears, gradually increasing until the sensation becomes converted into a combination of the most acute itching and smarting, with a feeling as though a number of small points were darting into the eye-ball; the eye at this time becoming inflamed, and discharging copiously a thick mucous fluid. This state of the eyes comes on in paroxysms, with uncertain intervals, from about the middle of June till the middle of July; the violent paroxysms never occurring more than two or three times daily, lasting an hour or two each time. The paroxysms may generally be traced to certain exciting causes, as a close moist heat, a bright glare of light, dust, and other substances stimulating the eyes. After these ocular phenomena have continued for a week or ten days, a general fulness is experienced in the head, to which succeeds irritation in the nose, with strong paroxysms of sneezing. To these are soon added a sensation of tightness of the chest, and difficulty of breathing, with a general irritation in the fauces and trachea, but without any absolute pain in any part of the chest. To these local symptoms are added, a degree of general indisposition; langour; incapacity for muscular exertion; inappetency; emaciation; restless nights; often attended with profuse perspirations; the extremities, however, being generally cold; the pulse permanently quickened, from 80 to 100 or 120.

The above may be considered a severe attack; and after such, the patient has generally better health than usual in the autumn. Almost every kind of medicine has been tried, with little or no benefit. “By using every means of obtaining fresh air, without much exertion, and by carefully avoiding a moist and close atmos-

^{*} Case of a Periodical Affection of the Eyes and Chest. By John Bostock, M.D.—Med. Chir. Transactions, vol. x.

phere, the symptoms may, in some measure, be kept off; but they have frequently appeared under circumstances that seemed the least likely to have procured them."

We have not the smallest doubt that the above complaint is one of those anomalous forms which irregular gout masks itself under, in particular constitutions. We have seen a case very similar to the above, only much more frequent in repetition, and severe in degree, in a female. We tried a great many remedies, and at length some rough ones. The disease disappeared for a time, but alarming symptoms of organic affection of the womb succeeded. The old complaint once more became reinstated, and the uterine symptoms instantly gave way. Barthez and Guilbert relate instances somewhat analogous to the above, where concealed gout played upon various tissues and organs in the most unaccountable shapes. We would, therefore, advise the narrator (if he be the patient) to *respect* the present manifestation of the complaint, and to rest contented with a mitigation of the malady by the prophylactic measures which he has found useful, rather than aim at the entire removal by *medicine*, of so long established a train of phenomena in the system, at the risk of incurring other and more dangerous affections.

§ II. NECK.

VI. *Laryngitis*.* There are few diseases more distressing in their appearance, or fatal in their consequences, than inflammation of the larynx. The delicate membrane lining the complicated structure of this part is exposed so constantly to atmospheric transitions, that it is wonderful the complaint is not more frequent, in this climate, than we actually find it. It has of late years, however, pressed more on the attention of the faculty than

* A Case of Chronic Inflammation of the Larynx, in which Laryngotomy and Mercury were successfully employed. By Marshall Hall, M. D. of Nottingham.

formerly, and it is one where medical and surgical skill is eminently beneficial, when promptly applied.

Laryngitis acuta makes quick work with its victim; and unless the young practitioner's mind be already stored with the necessary resources upon such an occurrence, he stands a chance of losing his patient, and probably a portion of his reputation, which he may be ill able to spare during the first years of his professional progress. On this account, we shall take the present opportunity of introducing some practical observations on the disease in question, before we analyse the case at the head of this article.

Laryngitis is an inflammation of the mucous tissue lining the various parts which compose the interior of the larynx, the glottis, and epiglottis. The patient feels an acute pain in that part of the throat, on attempting to swallow or speak, with a stridulous voice, short dry cough, great difficulty of breathing, redness of the face, prominency of the eyes, and, of course, symptomatic fever. It may terminate in three or four different ways—in resolution, by prompt and decisive measures; in sudden death, by suffocation, from the tumefaction of the parts; in the complete disorganization of the mucous membrane of the larynx, by mortification; in an ulceration of some portion of the larynx, ending in laryngeal phthisis; or, finally, in a partial obliteration of the rima glottidis, of which we have stated a memorable instance in a former number of this Journal.*

The exciting causes of this dangerous disease are almost invariably atmospherical transitions, or the application of cold or wet to some portion of the surface of the body, when hot or perspiring. We believe that a syphilitic taint in the constitution is rarely more than a predisposing cause of this affection.

If we see the patient before the inflammatory stage is past, which will generally be the case, (for people are not very slow in summoning the doctor, when they find themselves threatened with suffocation) we must put in force not in lingering succession, but in simultaneous co-operation, if possible, every means which medicine and surgery can suggest for the speedy reduction of this de-

* Vide P. 119 of our first Number.

structive inflammation. Every medical man, whether physician or surgeon, should carry a lancet in his pocket, and be ready to open a vein, on an emergency; for the College of Physicians have wisely imposed no oath against phlebotomy, as Hippocrates did against lithotomy. Whoever sees the patient first, then, in laryngitis, should instantly open the largest vein he can find in his arm, and abstract blood till *deliquium animi* suspends the flow, while leeches and blisters should be procured with all possible speed. Mean time, a large dose of calomel should be given, and followed by such other purgatives as are most ready at hand, or easily swallowed. As soon as the leeches arrive they are to be applied in clusters all over the throat, while a large blister is laid on, from the leeches, over half the sternum. During these transactions the warm bath is to be preparing; and, as soon as ready, the patient is to be immersed up to the scrobiculus cordis in it, and bled again, if no alleviation followed the first venesection. The patient should be directed to breathe, as much as possible, through the spout of a tea pot, containing warm water. When the purgatives have acted briskly on the whole line of the intestinal canal, the tartrate of antimony and digitalis should be exhibited so as to reduce the action of the heart, and produce nausea at the stomach. If these measures, reiterated as symptoms may require, do not reduce the inflammation, and relieve the breathing, we may prepare for bronchotomy, for no other means will save the life of our patient. By these means, we believe that we have saved some lives; and in one case which, in a subsequent attack, required bronchotomy, we completely dissipated the most violent and acute laryngitis that ever came in our way.*

But to the case before us. Mrs. Hatton, ætat. 53, became affected in September 1817, with hoarseness and hard dry cough, which continued to augment for two months. About the middle of November, a degree of difficulty of breathing, referred by the patient to a "tightness in the throat," was superadded. She now discovered that she was unable to "snuff up" through the nose in inspiration, in the ordinary way. All these symptoms went on in

* See Medico-Chirurgical Journal, (Monthly Series) vol. v. page 1.

aggravation till the beginning of February, 1818, when a degree of dysphagia was experienced. In March, she observed a rather diffused swelling, about the size of a pigeon's egg, over the superior part of the thyroid cartilage, with an increase of the dyspœna and dysphagia. Some temporary benefit was derived from a liniment; but the symptoms again exasperating, continued to augment till the month of August. During this period, the patient constantly referred the seat of her difficult breathing to the upper part of the larynx. Her cough, at first dry, harsh, and croupy, became latterly attended with a viscid expectoration, tinged with blood. On the 15th of August, Dr. Hall ordered five grains of the quicksilver pill, night and morning; half an ounce of the sulphas magnesiae twice a week; four leeches to the larynx every other day; and the parts to be kept wet with a lotion, when the leech bites were not bleeding. 22d of August. There being no amendment, the mercury was given thrice a day. On the 24th, she was seized with an alarming fit of dyspnœa, with great anxiety of countenance, &c. but it gradually subsided to her usual state.

In consultation with Mr. Oldknow, a skilful surgeon of Nottingham, laryngotomy was determined on, and performed by Mr. O. in the following manner. An incision being carried from a little above the thyroid to below the cricoid cartilage, the external surfaces of these were laid bare, partly by the knife, partly by the fingers. An opening was then made between the two cartilages; but this not being sufficient for respiration, a *crucial* incision was made in the membrane connecting the two cartilages. The result being still unsatisfactory, a circular portion, about one-eighth of an inch in diameter, was removed from the lower and lateral part of the thyroid cartilage, when the respiration became free, and the patient was relieved. The cut edges of the integuments were kept asunder by straps of adhesive plaster.

In this operation there was some difficulty experienced from the inability of the patient's reclining backwards her head. The larynx could not, in consequence, be brought sufficiently forwards, "and the depth of the incision was necessarily greater than was contemplated." In some judicious observations on this operation, Mr. Oldknow appears to think that an opening between the

thyroid and cricoid cartilages possesses advantages over one made lower down in the trachea, "especially when we consider the impossibility frequently experienced of employing a tube" for a permanent opening. "In the present case, the introduction of a probe, merely, induced the most distressing fits of convulsive coughing." The case which we have alluded to above, shows the possibility of keeping a tube permanently in the trachea; whereas there seems little or no chance of the same being possible in the larynx, from the greater sensibility of the parts. We have repeatedly passed large bougies down the trachea through the tube in Mr. Price's case, and on no occasion could he ever feel the bougie. It would sometimes, indeed, set him a coughing, from the *organic* sensibility of the mucous membrane; but he could never tell why he coughed, as the membrane possessed nothing of that kind of sensibility residing in the nerves of *touch*. We sometimes endeavour to pass the bougie *upwards* through the larynx, to explore the cause of the obstruction; but this always brought on violent *sensible* irritation, and long paroxysms of coughing. Where, therefore, there is probability that a *tube* may be necessary at all, we think the trachea the preferable place for the operation, and that as low down as possible, consistent with safety.

M. Jourdan, in commenting on the preference which Chopart and Desault gave to laryngotomy over tracheotomy, comes to the same conclusion which we have done. "Mais il est difficile, (says he,) de concevoir quels avantages elle (laryngotomie) peut avoir sur la tracheotomie. Si d'ailleurs on considere que le sommet de la trachée en est toujours la partie la plus sensible et la plus irritable, on se décidera sans peine a la reserver exclusivement pour la cas ou des circonstances qu'il est impossible de prévoir d'avance, empêcheraient de pratiquer la tracheotomie." *Dict. des Sciences Med. Vol. xxvii. p. 273.*

Mrs. H. after this operation slept soundly the ensuing night; but deglutition continued difficult, and always induced coughing during the five or six succeeding days. The voice, of course, was lost. On the day after the operation, mercurial ointment was substituted for the blue pill; and on the 28th, the mouth became sore.

Soon after this, Mrs. H. experienced a mitigation of the dysphagia; and, on applying the finger to the wound, she found that the difficulty of respiration was also diminished.

This amendment continued progressive, and on the 11th of September, the orifice closed, the respiration being free, and the swallowing easy. A few weeks after this, she imprudently exposed herself to cold, which was succeeded by a difficulty of breathing and swallowing, together with a loss of voice. She came once more to Nottingham; was put on the mercurial treatment, "and in proportion as the remedy induced ptyalism, the dysphagia disappeared entirely, and the voice became again improved." The medical and surgical measures pursued in this case reflect great credit on the practitioners engaged therein; and we cannot terminate this article better than in the words of Dr. Hall.

"It may, I think, be fairly concluded, that by means of laryngotomy and of mercury, this poor sufferer was saved from an inevitable death. This case affords, therefore, a striking instance of the efficacy of *modern Medicine and Surgery*; for it is but justice to state, that the means employed were adopted principally on the suggestions of living authors, and especially of those whose names have been already mentioned."*

VII. *Bronchocele*.† We had drawn up an analysis of this paper; but just as it was going to press, we received intelligence from a friend at Naples, that the practice of Quadri was found totally unsuccessful, and had been entirely given up. We observe that this is corroborated by Dr. Clark of Rome, in his work reviewed in our present number. We feel it our duty, therefore, to suppress the analysis and lose our labour, rather than contribute to the propagation of an unsuccessful experiment, and uselessly usurp space that may be better employed.

* Baillie, Farre, Lawrence, Charles Bell, &c.

† Memoir on a new Mode of Treating Bronchocele. By Dr. Quadri, of Naples. *Medico-Chirurgical Transactions*, vol. x.

§ IV. ABDOMEN.

IX. Ileus.* That the establishment of truth, and the advancement of medical science, are the paramount objects of Dr. Abercrombie, we sincerely believe. That this able physician and surgeon will give us credit for similar feelings and impulses, we have no reason to doubt. Under this impression, we shall investigate the subject of the paper in question, with a degree of attention proportioned to the importance of the disease, and the respect which we entertain for the author. If we combat Dr. Abercrombie's theories, it is not with the intention of depreciating his facts; if we endeavour to develop a sounder pathology of the disease, it is for the sole purpose of co-operating with the author in establishing a rational mode of treatment.

We conceive that the pathognomonic or essential character of ileus, is an *inverted motion or action of the intestinal canal*—in other words, that the simplest form in which the disease can exist, (and unfortunately the rarest) is this inversion, unaccompanied by any obstruction in the intestinal tube. Sennertus relates a case of this kind. “*Observavi adhuc aliud ilei et motus intestinorum inversi genus, neque inflammatione, neque ulli causarum enumeratarum, in quodam viro hypochondriaco; in eo enim ita humores sursum ex hypochondriis vergebant, &c.—ut clysterem vomitu rejiceret.*” The following case, from Barthez, is also in point. “A man became afflicted with pains in the stomach and bowels, which daily increased in violence, till they at length deprived him of rest. One day, after a very slight repast, he was seized with a painful spasm in the epigastrium, which was quickly followed by vomiting and convulsive efforts. *Lavements being exhibited he threw them repeatedly up by the mouth.*” It was cured by antispasmodics, leeches, blisters, &c. Morgagni, in referring to the histories of such cases, says, “you will find among the several observations, that the glysters were thrown up by the mouth, wholly,

* Researches on the Pathology of the Intestinal Canal. By John Abercrombie, M. D. Ed. Journal, January, 1820.

entirely, purely as they had been applied, nothing at all changed, after they had been in the intestines for an hour, a quarter of an hour, and sometimes only for a few moments after they were exhibited." xxxiv *Epistle*.

Not to multiply authorities on this point, the fact is authenticated by Van Swieten, De Haen, and numerous authors of undoubted veracity.* Suppositories even have been rejected by the mouth; a curious case of which is related by Mathieu de Gradibus. "A girl, 12 years of age, was attacked with ileus. The constipation was obstinate, and she threw up fecal matters and glysters entire (elle rejeta par le vomissement les matiers fœcales et des lavemens entiers.) These symptoms continuing three days, a long suppository was introduced per anum. It was quickly drawn up into the intestines, and rejected by vomiting." "Aussitot il remonta dans les intestins, et fut rejeté par les vomissements."†

The above, we should hope, are sufficient to establish the point, that obstruction is not essential to the production of simple ileus; else how could glysters and suppositories pass rapidly from the rectum to the mouth, even in defiance of the valve of the colon.

But how, and why, is this inverted or anti-peristaltic motion produced? Not surely in the way Dr. Abercrombie has pointed out—namely from a portion of intestine being "distended beyond its power of contraction, or paralysed from over distention;" and that too, by the increased impulse of matters from *above*.‡ In short, the facts brought forward here, are fatal, at once, to the

* Among others, Sydenham, no mean authority, as a faithful observer of Nature observes, "quæcunque in intestinis continentur, non versus alvum, sed ventriculum protruduntur, et impetu facto, and os regurgitant, adeo ut enemata quantumcumquæ acria, emetica evadunt." Chap. iv.

† See the account of this case also, in the 3d Book and 14th Section of Bonetus, where he relates some instances himself of a similar tendency.

‡ It has been said that there is nothing *new* under the sun. Notwithstanding this dogma, we were inclined to give Dr. Abercrombie full credit for originality in this doctrine, till we read in the 34th Letter, and 30th Article of Morgagni, of "those cases in which ileus has happened on account of the *expulsive* faculty being abolished, or from a *loss of tone* in the intestines, according to the opinion of Salius and Ruysch." We cannot say that this accession of authority, however, has occasioned any change of sentiment in our minds.

Doctor's hypothesis, as far as regards what we term *simple ileus*, in which his supposed obstruction from over distention, accumulation, and paralysis cannot exist. Let us endeavour to account for this inversion on other principles. We ourselves have seen some instances of the simple ileus, and our reflections on the subject had long ago led us to attribute the anti-peristaltic motion to some morbid irritations of the *nervous system*, which play a greater part in pathology than some modern physicians are inclined to allow. This opinion was not a little strengthened by a perusal of the following facts. "Schwartz, says Barthez, has proved by direct experiments, that this inversion takes place and produces vomiting, if we prick various parts of the brain, the fifth pair of nerves near their origin, or the mesenteric plexuses; and Brunner, by irritating the intestines of divers animals, has excited in these canals convulsive motions, which caused their excrementitious contents to retrograde into the stomach, whence they were discharged by vomiting." "Barthez rapelle que Schwartz a prouvé, par des experiences directes, que cette inversion peut avoir lieu et produire le vomissement, lorsqu'on pique divers endroits du cerveau et du cervelet, ou les nerfs dits de la cinquième paire pres de leur origine, ou les plexus mésentériques; et que Brunner, en irritant les intestins, meme dans divers animaux, y a excité des convulsions qui ont fait remonter les matières excrementitielles dans l'estomac, d'ou elles ont été chassées par le vomissement."*

The more scrupulously we examine the phenomena of this simple form of ileus, the more we will be convinced that these inverted actions result from irritation applied either to the origins or extremities of the nerves supplying the intestinal canal. Sydenham, whose practice was bad in ileus, entertains some just notions respecting its etiology. "Hujus inversionis (says he) unde dolor exoritur, *duplicem causum*, adsignare licet. Obstructionem, nempe, vel irritationem." Chap. iv.

The next least complex form of ileus, is that in which there is a spasmodic constriction, and consequently obstruction, more or less fixed, in one or more points of the intestinal canal, most com-

* Monfalcon sur l'Ileus.

monly in the ileum, without inflammation, or any change of structure.

Dr. Abercrombie doubts whether the muscular fibres of the stomach or bowels are capable of being affected with spasm to a morbid degree. But really we see nothing in his arguments to justify this extraordinary conclusion. He first tells us, page 2, that the peristaltic action of the intestines has nothing analogous in any other part of the body; but at page 6, he seems to forget this, for he there tells us, that "our pathology of such muscles must be derived chiefly from the urinary bladder." This is strange reasoning! But first let us ask—What is this spasm which is denied to the intestinal canal? We believe that our best physiologists consider spasm as nothing more than a morbid, and generally a painful extreme of contraction; the opposite extreme of which is paralytic relaxation. Dr. Abercrombie admits the *latter* extreme, but denies the *former*. We shall, at present, avoid the fallacious ground of analogy, and beg leave to take Dr. Abercrombie on the more solid basis of *the symptoms during life, and the appearances on dissection*.* No man who has seen a case of ileus, or even common colic, will deny that the symptoms of spasmodic pains are infinitely more prominent than those which might be supposed to attend a paralytic relaxation of the intestines. Even if the pain in ileus were, in a great measure, owing to the unnatural distention, by flatus or other matters, of some part of the bowel, still we contend that this distention would not exist, were it not for a constriction some where else. But the patient dies. We find one portion of ileum unnaturally distended, and another contracted, so as to resemble a cord. We would ask every un-

* Dr. A. instances the urinary bladder, when "distended beyond its power of contraction," as illustrating his doctrine of distention as the cause of ileus. But he takes no notice of the usual *causes* of this vesical distention; namely, spasm, inflammation, strictures, or other organic affections of the urethra or neck of the bladder, or too long voluntary retention of the urine. This part of the analogy very beautifully illustrates the pathology of ileus; but this part is entirely overlooked by Dr. Abercrombie! Dr. A. questions the existence of spasm in the bladder. As a practical surgeon, he must have seen spasmodic contractions of the bladder, from the irritation of calculi, &c. which sometimes become encysted in this way.

prejudiced observer, whether we have not as good reason to consider the *latter* portion of bowel in a morbid state of contraction, as the *former* in a morbid or paralytic state of dilatation? If this contraction be not always very firm, in the dead body, will any candid man assert, on this account, that no spasmodic constriction existed during life? But the fact is, this morbid contraction is so very generally to be seen, in fatal ileus, where no other visible cause is found, that it has given the name of *Chordapsus* to the disease, from the very appearance described by Dr. Abercrombie, ("a solid cord, white and corrugated,") as constituting the natural and *healthy* state of the intestine. "Nearly the whole track of intestine (says he) may occasionally be seen in this state in the bodies of infants, who, before death, had been *much purged*, or who had been affected with *diarrhœa*, without disease of the coats of the intestine." Dr. A. does not consider that the irritation of purgatives, or the irritable and sub-inflamed state of the mucous membrane, in *diarrhœa*, has any share in producing this *chordapsus* of the intestine. He considers the muscular fibres in this greatest degree of *contraction*, as being probably, in the greatest degree of *rest*! Upon such principles, it is needless for us to attempt to reason. Neither shall we vindicate the *supposed erroneous* doctrine that spasm cannot exist in the stomach, against so inconclusive an argument as Dr. Abercrombie has brought forward against it, namely, that "we find persons labouring under the affection which has received this name, swallowing

* Dr. A. refers to the urinary organs for analogies; he cannot, therefore, refuse the following: "This contraction and relaxation (says Sir Everard Home) form the natural and healthy actions of the urethra; but as this membrane, like every other muscular structure, is liable to a *spasmodic* action, which produces a degree of contraction beyond the natural, in which state the canal loses the power of relaxing, until the spasm is removed. When this happens, it constitutes a disease; and is termed a spasmodic stricture. While a stricture is in this stage, it is only a wrong action of the membrane of the urethra; and if the parts are examined in their relaxed state, there would be *no appearance of disease*." If this be true of the urethra, where muscular fibres can hardly be demonstrated, how much more applicable is it to the intestines? Spasm then *may* be relaxed by death, and leave no trace of its existence.

hot water and other liquids in large quantities." p. 6. We would only just venture to hint, from some little observation and personal sufferings, that *partial* and very painful spasmodic contractions may obtain in the stomach, as well as in other parts of the intestinal canal, without such an obliteration of its cavity as would prevent a man from swallowing hot water and other liquids. But to return to the direct path of investigation. The natural and healthy motions of the intestinal tube, are not only continually varying the calibre of its different parts, but contorting the convolutions of the small intestines themselves, so as to be every moment forming twisted strictures, and perhaps even slight intussusceptions, that are as quickly unravelled by the next vermicular movement. But let a morbid irritation be propagated to, or generated in this writhing tube, and then we shall have *spasmodic* contractions, instead of natural ones, attended with colicky pain, intermitting or remitting; if the spasms intermit or remit, nausea, vomiting, and all the phenomena of ileus. This spasmodic constriction may supervene on simple ileus, (the antiperistaltic motion of the bowel;) or it may produce ileus, and in either case, we have a combination, more formidable than the simple inverted action of the intestines.

In this kind of constriction, the antiperistaltic motion is not constantly going on. On the contrary, it appears to take place only when the natural action of the bowels has distended the parts above the obstruction to a certain extent, with the matters contained therein, when the inverted motion really appears an effort of nature to void upwards, what cannot be passed downwards. That ileus, so little complicated as in this form, often destroys life, we very much doubt. Dr. Abercrombie's first case is related as an example;* but it is not very circumstantial; and, at all events, we know that so slight a degree of inflammation in the bowels, as is scarcely cognizable on dissection, will sometimes destroy life. In Dr. A's case, the bleeding and other antiphlogistic measures, too, might have prevented the traces of inflammation being very evident. Dr. A. indeed, acknowledges

* Edinburgh Journal, page 8.

that this is "an unusual termination of the disease in adults;" but believes that infants are frequently cut off in this manner, by the mere interruption of the healthy action of the intestinal canal.

The *third* form, or rather complication of ileus, which we would advert to, is that in which inflammation is added to spasmodic constriction and antiperistaltic motion. This is by far the most frequent form which we meet in practice, though the *second* form probably precedes it, in most instances, before we see the patient. Between violent spasm and inflammation there is a very thin partition, which every practitioner ought to bear in mind. We believe that ileus rarely proves fatal without inflammation, when, in fact, we have enteritis to contend with, in addition to the original disease. The following passage is well worthy of quotation; and we would particularly solicit Dr. Abercrombie's attention to it. "*Quippe cum plures ab ileo defunctos aperuerim, intestini cujusdam inflammationem, ac sphacelum fere in omnibus, et morbi et mortis causam fuisse deprehendi, neque mirum hoc, quoniam in membrana tenera et maxime sensili magna solutio continui spasmos et corrugationes dolorificas ita continuas et atroces ciebat, propterea ut intestini affecti motus peristalticus, quo fæces alvi versus anum protruduntur inhibeatur ac prorsus invertatur.*" *Sepulchret. Bonet. lib. iii. sect. xiv. observat. xix.*

We do not believe that a sounder piece of pathology, in respect to ileus, has been broached since the days of Bonetus, or one which is in stricter unison with the best principles of practice, as we shall see farther on. The writings of Morgagni, Lieutaud, Bonetus, and all pathologists are full of histories of this form of ileus; and they all concur in shewing not only obstruction but inflammation in the intestinal canal. We shall not, therefore, take up space in relating individual cases, but reserve it for more useful and general observations. We cannot, however, pass unnoticed some specimens of what Dr. Abercrombie calls ileus without obstruction; because we think that they are not only misnomered, but misplaced, as bases for a false hypothesis.

In the seventh case, page 13, the lady "was affected with vomiting and pain over the whole abdomen, which was rather

tense upon pressure; pulse rather frequent. She was bled, and blistered, and took laxative medicine, which operated freely." Surely Dr. Abercrombie is not in earnest, when he adduces the above as a specimen of ileus. Is the simple act of vomiting, a proof of the peristaltic action of the intestines being inverted? And are pain and tension of the whole abdomen the symptoms of a local paralytic distention, on his own principles? The fact is, that this patient had a mass of hardened fæces in the colon, which was producing general abdominal irritation, threatening inflammation, with sickness at stomach of course. After bleeding, blistering, and subsequently wine, in considerable quantities, these hardened fæces were brought off "in most extraordinary masses," by injections and castor oil, when she was immediately made well. Where was the ileus in this case? Dr. A. says it was a case in which there was no obstruction. We cannot agree with him. The first purgatives only brought off "thin fæces," which did not relieve the patient. The masses of hardened fæces must necessarily have produced obstruction, though not a total stoppage of the passage. Had the latter taken place, real ileus would, in all probability, have supervened.

"CASE 8th. A gentleman, aged about 40, (10th November) was seized with vomiting and pain of the left side of the abdomen, his pulse varying from 40 to 60; took purgative medicine, which operated fully; and on the 11th, the vomiting had subsided, but the pain continued severe, and was more general over the abdomen; pulse 70.—12th. A tympanitic swelling appeared on the left side of the abdomen, which, on the 13th, had extended also to the right side; violent pain continued; pulse natural.—On the 13th, he took purgative medicine, which operated fully four or five times.—On the 14th, he was free from pain; but the swelling had extended over the whole abdomen; pulse still natural. On the 15th, the pain returned with great violence, with vomiting, and frequent pulse. It continued through the day and night, and he died early on the morning of the 16th. The body was not examined."

As there was no dissection, we will not take upon us to say positively what the disease was, (though few will hesitate to pro-

nounce it peritoneal inflammation with tympanites) but we affirm that it *was not* ileus. There was no evidence of inverted motion of the *bowels*, for stercoraceous vomitings are not even hinted at; and from the 11th to the 15th, there was not even inverted action of the *stomach*. There is no evidence of obstruction in the gut, for purgative medicine "operated fully four or five times." If the tympanitic distention then was within the bowels, it must have been gaseous; and what prevented it from passing off, if, as Dr. A. acknowledges, there was no obstruction?

We may remark, "*en passant*," that this patient could not have been Dr. Abercrombie's, for the treatment was exceedingly inert. He, therefore, probably had but an imperfect account of the case.

Dr. Abercrombie's next case, [Case 9, p. 13] has still less pretensions to the name of ileus. There was no vomiting or antiperistaltic motion of the bowels—no accumulation—no obstruction. The case was clearly spasmodic colic ending, as it often does, in inflammation and death. On the 11th of June, 1818, a gentleman was attacked with "violent pain across the lower part of the abdomen, which was *drawn up into balls*." These attacks, he used to have periodically, sometimes returning every evening, for a week together. He was relieved, and his bowels evacuated by an *opiate and purgative*. Two days afterwards, the pain returned with violence, and was not relieved by an opiate. He was not bled till the next morning, by which time spasm had ended in inflammation and gangrene. On many parts of the ileum there were inflamed portions, with effusion of coagulable lymph, and others of a dark colour, approaching to gangrene. An old adhesion between the caput cæcum and ascending colon, evinced that in former attacks inflammation had not been wanting occasionally, though never to a fatal extent. There is not the least shadow of reason for placing this case under the head of ileus.

It is to be remembered that as, in this third form of ileus which we have been examining, inflammation supervenes on spasm and antiperistaltic motion of the intestines; so, on the other hand, the antiperistaltic motion itself, or ileus, very often super-

venes on inflammation, and thus becomes a secondary phenomenon attending enteritis. This appears to have led Dr. Abercrombie astray; for he has evidently confounded ileus, enteritis, and spasmodic colic, all under one denomination.

The fourth and last form or complication of ileus, is that in which there is organic or mechanical strangulation of the intestine, as hernia, intus-susception, volvulus, foreign bodies in the tube, and various other causes of obstruction. The histories of such cases are endless. It is proper to state, that in *these* instances, the symptoms are the same as in all the other complications of ileus. The distention, too, of the bowel above the *organic* stricture, is the same as above the *spasmodic* contraction; a plain proof that the said distention was caused by, and did not cause the contraction. We shall make a few observations on some of the principal organic or mechanical obstructions, which supervene on or produce ileus, passing over strangulated herniæ, as too familiar to every reader.

1. *Intus-susception.* This, as we before hinted, probably takes place, in a slight degree, during healthy peristaltic action; but the succeeding vermicular movement undoes what the preceding had done. In severe colics, or where there is much nervous irritation, however, the intus-susception may become fixed, an obstruction produced, adhesive inflammation kindled, and death ensue. In this way, we can see that it may either supervene on ileus, during the inverted action of the bowels in that complaint, or it may precede ileus, and produce it. Peyer, in a great number of experiments, demonstrated the possibility of inducing intus-susception, by irritating the intestines of animals; and, in all human probability, it is in this way, namely, by irritation, that the accident takes place so often in children, whose bowels so generally contain irritating substances and secretions, and whose nervous systems are so susceptible of various morbid sympathies. There is reason to believe, from many cases on record, that these invaginations are sometimes caused by worms. We shall only introduce one instance as an example. M. Raisin relates the case of a soldier, who, in the period of convalescence from a fever,

was seized one night with inexpressible anxiety, and unconquerable restlessness. Presently he raised himself up in his bed, gave vent to some groans, and fell down dead. On dissection, the epiploon was found slightly inflamed, as were the intestines, which were amazingly distended with gas. An intus-susception, of about four inches in extent, was found at the superior part of the ileum, above and below which were seen a number of lumbrici.

Although there is no diagnostic symptom by which we can ascertain intus-susception with any degree of precision, yet Montfalcon thinks that "the violence and fixed seat of the pain, the obstinacy of the constipation and vomiting, together with the rapid prostration of the patient's strength, are symptoms strongly indicative of intus-susception." Even in this dangerous situation, nature sometimes delivers the patient, when art is completely unavailing. Hévin has collected many authentic examples of mortified intestine being thrown off per anum. In one case, (*Observations de Sobaux*) twenty-three inches of colon, with the part of mesocolon to which it was attached, were expelled by stool; in another, (*Observations de Salquer*) twenty-eight inches of ileum, in a state of gangrene, were thrown off; in a third, (*Observation de Fauchon*) the cæcum with six inches of colon, and the same of ileum, were discharged per rectum, and in all, the patients perfectly recovered. Dr. Baillie has also recorded a similar event, but we believe the patient died about three weeks afterwards.

2. *Volvulus—Entortillement—Entérelésie.* It is by no means uncommon to find some part of the small intestines so twisted, interlaced, or, as it were, involved in a knot, as to produce complete strangulation, with or without adhesion, inflammation, or gangrene, according to the length of time which the patient had been able to bear up against the complaint. We shall illustrate these by a few examples.

"A youth of 17 years, vomited stercoraceous matter during fourteen days before his death, without passing any thing by stool the while. When the abdomen was opened, a great quantity of

gas rushed out. The intestines were vastly distended; some of them as thick as a person's thigh, in certain places" "Aliis vero locis adeo convoluta, intorta et implexa, ut nulla illic nec excrementis nec flatibus deorsum pateret via: sed et vermibus vivis oblongis, quamplurimis repleta erant qui rursum aliis minoribus referti, inequales illos motus efficerant." *Platerus. Obs. Lib. 3.* "Testisque mihi fuit **TILEMANNUS**, se vidisse aliquando in Sectione Anatomica ab ileo mortui, intestinum ileum contortuplicatum, uti pannus à lotricibus convolutus." *Dietericus Iatrei.*

The following most interesting case we shall translate from Riverius.

"January 29th, 1644. D. Patris, began to complain of violent colicky pain, as was supposed, by exposure to intense cold. The family apothecary threw up a glyster, which brought away much fæces. The pain continuing, two or three other glysters were exhibited, but returned unaccompanied by stools, the violence of the pain still increasing. The next day, Riverius was called in, and prescribed anodyne fomentations and glysters. There was now neither pain nor fever; but the patient vomited at intervals, and no passage could be procured by stool. The disease, in a consultation, was considered to be ileus. On the third day, there was fever, with dry tongue; but there being no pain, it was thought there could not be inflammation of the bowels. The cause was thought to be accumulated fæces, or a contortion of some part of the intestine. Bleeding was thrice performed, glysters were thrown up, and various remedies were tried; but the vomiting and constipation continued till the seventh day, stercoraceous matter being thrown up by the mouth. On the evening of the seventh, the bowels were opened, great quantities of fæces passed by stool, and the vomiting stopped. A diarrhœa continued for five days more, which becoming colliquative, the patient was carried off on the thirteenth day of his illness."

What were the appearances post mortem here? Had no dissection taken place, it would be confidently asserted, that there was no organic obstruction, as the open state of the bowels seemed to

prove. Let us see. On opening the abdomen, "*inventum est intestinum ileon tribus complicationibus convolutum, et quasi compactum in unam massam, circa finem illius. Tota autem illa intestini portio complicata, gangrena affecta erat, cum portione mesenterii sibi adjuncta.*" The rest of the intestines were greatly distended. The ileum, above the obstruction, had burst, "*à pondere materiæ contentæ in superioribus intestinis,*" and the excrementitious matters were extravasated into the abdomen. These last had burst into the rectum, and were discharged by stool for several days; consequently, as Riverius justly observes, "*obstructionis intestinorum apertionem mentiebantur:—unde patet alvi fluxum, ileo superveniente, posse interdum medico imponere.*" *Centaur. 3. Observat. 26.*

This case shews us how difficult it is to ascertain the period of transition from spasmodic or any other obstruction to inflammation, and how dangerous it is to keep any other hypothesis in view than the possibility of inflammation.

The appendix ceci vermiformis has been known to occasion fatal ileus by strangling a contiguous portion of intestine. Vermiform appendices of this kind are sometimes found on the small intestines, and two instances of their causing ileus now lie before us. The one happened at Lyons, and is stated by Dr. Martin of the Hotel Dieu there. A young man entered the hospital with pain and tension of the abdomen, which took place immediately after lifting a heavy burthen the evening before. He felt, at the time, something crack in the abdomen. All the symptoms of ileus quickly supervening, he died on the sixth day. On opening the abdomen, the intestines were found greatly distended with gas, and the major part of the ileum sphacelated. A vermiform appendix, very similar to that of the cæcum, taking its origin from the ileum, had its other extremity adherent to a portion of neighbouring mesentery, and thus formed an arch over three knuckles of intestine, which were completely strangulated thereby.

The following case, by Renault, evinces the rapid march of inflammation in these mechanical obstructions, which exhibit the same symptoms during life, and the same appearances on dis-

section, that the spasmodic constrictions do—a convincing proof, that it is obstruction, not paralysis, which is the fundamental cause in both species. “A man felt pain in his bowels, which became greatly aggravated in the night, and next day was succeeded by all the symptoms of iliac passion. He died on the second day of his illness. The abdomen was prodigiously distended; and, on being opened, a great quantity of fœtid gas rushed forth. There was considerable effusion of dark coloured serum into the abdominal cavity. Various portions of peritoneum and gastric epiploon were gangrened. The small intestines universally inflamed; and, in some places, sphacelated. A vermiform process springing from one of the small intestines, seven inches in length, formed a knot around, and strangled a knuckle of mesentery.”*

This case elucidates that tympanitic distention which so generally attends peritoneal inflammation, seemingly occasioned by the extrication of fœtid gas. An ingenious explanation of this phenomenon was read, a few years ago, before the Royal Society, by Dr. Granville, some account of which may be seen in the Journal of the Royal Institution, for July 1818. Dr. Granville, in a late publication,† states, that he has now under his care a case of that rare disease, œdopsophia, or “*flatuum ex utero per vaginam, emissio*,” which he considers as of the same nature as tympanitis. “I am strongly tempted* to believe, says this ingenious and erudite physician, that the extricated air in this case is a combination of sulphur and azote, from a decomposition of the albumen of the blood, in consequence of inflammation of the womb. This fact I was the first to notice in a communication read before the Royal Society in May 1818, giving an account of a new compound gas, produced by peritoneal inflammation. In both cases, the albumen is separated from the blood by the process of inflammation, and is ultimately decomposed by some other process, yet unknown, giving rise to carbonic acid gas on the

* *Dict. des Sciences Med.*

† Report of the Practice of Midwifery at the Westminster General Dispensary. By A. B. GRANVILLE, M. D. London, 1819.

one hand, and to sulphuretted azote on the other; producing, as in the former case, *ædopsophia*, the disease here mentioned: and, in the second, abdominal tympanitis." *Loco citato*.

We may here just observe, *en passant*, that the distention within the intestines, *found on dissection*, should be regarded with a suspicious eye; and, in our idea, it affords a very unstable basis for an etiological hypothesis. Every one knows what an immense extrication of gaseous fluid takes place in the bowels, immediately the vital principle becomes extinct. And as this is generally the case in the intestines, for some time prior to the death of the patient in ileus, so there is every reason to believe that the greater part of the distention, seen in dissection, takes place at a very late period of the disease, or even after death; and so far from being a cause, it is quite a secondary, or ternary effect of the morbid state in ileus.

M. Fages relates an interesting case of fatal ileus, caused by a portion of ileum becoming incarcerated in a sac of peritoneum, situated on the anterior part of the psoas muscle; and many instances are on record where the intestines have been strangulated by bands of the epiploon, and by knuckles of gut passing into rents of the same.

III. *Ileus from Fæces or foreign Substances lodged in, and obstructing the Intestinal Canal*. A girl, seven years old, was afflicted for several days with a diarrhœa, which the old women at length put a stop to by exhibiting large quantities of quince fruit. Very soon after this sudden suppression of the diarrhœa, she was seized with severe and painful tormina of the bowels, together with distention of the abdomen. The physician being called, endeavoured to restore the diarrhœa, but in vain; no stools could be procured; stercoraceous vomiting came on, and death ultimately closed the scene.

Dissection. "Aperto corpore, cæcum intestinum coangustatum constrictumque, adeo obhærescente, et interioremque ductum obturante cydoniato, deprehensum est, ut illac nullo modo prorsus quicquam posset pervadere." A little above the obstruction the intestine had given way, and fæces were extravasated in the belly. *Fenelius Pathol. lib. 5. Cap. 9.*

Lamartiniere relates the case of a young gentleman, who, for the purpose of stopping a relaxation in his bowels, indiscreetly eat a great number of hard boiled eggs. Unconquerable constipation followed, with obstinate vomiting, till death put an end to his sufferings. A column of intestine was found firmly impacted with hard substances, above which, the gut was enormously distended.

In the 4th volume of this Journal, (Monthly Series) Mr. Morrison, a navy surgeon, relates the case of two marines, who ate, at one meal, *forty-four* eggs. They both died; one on the second day, with ileus; the other on the eighth, with inflammation of the abdominal viscera, especially the stomach and liver.

Chronic strictures of the colon, and even of the rectum have induced ileus, of which we could adduce several examples. But we think enough has been said.

TREATMENT OF ILEUS.

We have endeavoured to shew that ileus presents itself, in practice, under four principal forms; viz.

1st. As an inverted or antiperistaltic action of the intestinal canal, without obstruction, spasmodic, inflammatory, or mechanical; dependent, as we imagine, on nervous irritation, general or local. This is the simplest form, and is so rarely met with, that its consideration can have but little influence on our practice. Indeed, we cannot positively decide on its presence, unless we see glysters vomited up; for numerous experiments prove, that if ligatures are made on the lower portion of ileum, the animal will still vomit *stercoraceous* matters, shewing that in such cases these matters acquire a *fæcal* smell and appearance, without having descended into the large intestines. We have, ourselves, seen two instances, where the glysters were so unequivocally thrown up from the stomach, as to leave no doubts in our mind; and we have proved the existence of this form by unquestionable evidence in the beginning of this paper. The treatment, in cases of this kind, is sufficiently indicated. Opium, united with calomel, is the principal remedy to be depended on; for the disease is, in fact, a cholera, the current of which is turned entirely upwards. The warm bath, opiate glysters, blisters to the abdomen, and blood-letting to guard against inflam-

mation, are also to be had recourse to, if the symptoms do not quickly subside.

2d. In which there is spasmodic constriction, (either as cause or consequence of inverted peristaltic action,) and consequently obstruction, more or less fixed, in some part or parts of the intestinal canal, unattended with inflammation or mechanical impediment.

3d. In which inflammation is superadded to the spasmodic constriction.

4th. In which there is mechanical strangulation, with or without inflammation, and whether preceding or supervening on the simple and spasmodic forms of ileus.

Now the practical point is, can these three last forms of ileus be distinguished at the bed-side of sickness? Between the two last, we believe no man, who knows the uncertainty of diagnosis, would venture to lay down a positive distinction, from the symptoms, during life; and between spasm and inflammation, *in the intestines*, there is, as we before remarked, a very thin partition! The diagnosis, indeed, is accurately laid down, *in books*, but we have seen it often lead astray *in practice*.

The symptoms common to all forms of ileus are, pain, more or less violent, and more or less fixed, in some part of the abdomen, generally about the umbilicus; obstinate constipation; vomiting. It is, we believe, by the degree of severity in these, and the *plus et minus*, in the other symptoms which arise, that the best practitioners judge of the nature and danger of the individual case. Thus, the less fixed and the less constant the pain is, so much the more favourable our prognosis. Provided there is a clear intermission, or well marked remission of pain, its great violence, we have generally observed, during the paroxysm, is more indicative of spasm than inflammation or volvulus, and therefore less dangerous, at the beginning; but in violent spasm of the intestines, as we before remarked, inflammation is ever imminent, and ever to be dreaded. The pulse is exceedingly fallacious in all intestinal affections; and the absence of pyrexia, or even pain, *while no stools can be procured*, is never to lull us into a false security.

In common *enteritis*, the pain is more diffused than in ileus; the

constipation is less obstinate; the vomiting supervenes on the inflammation; is less violent, and rarely brings up *faecal* matters; indeed it appears to be merely an inverted action of the stomach, from sympathy with the intestines, and is a common phenomenon, or rather epiphenomenon, in peritoneal inflammation also.

If the ileus results from *faecal* accumulation, an accurate examination of the abdomen will generally detect it, and the history of the case will lead us to suspect the circumstance. It is almost needless to say, that the groins should always be carefully examined, lest a strangulated hernia be the cause of the symptoms. Monfalcon lays down the following train of phenomena, as indicative of strangulation of the intestinal canal from an internal cause. "Painful tension of the abdomen; obstinate constipation of the bowels, on which the most irritating lavements have no effect; hiccup; nausea; vomiting, at first of half digested aliment, and ultimately of stercoraceous matters; extreme general uneasiness; pulse small and cordy; cold, clammy, and partial sweats; coldness of the face and extremities; great alteration in the countenance; hollowness of the eyes; tumefaction of the abdomen; pain, more or less violent, in some part of the belly. When the respiration becomes feeble, with tendency to somnolency, and a sudden alleviation of the pain, death is at hand." The above symptoms indicate, with sufficient certainty, an obstruction of some portion of the intestine; but the exact nature of this obstruction no man can positively say.

We shall now proceed directly to the treatment. In the simple, but unfortunately not common, form of ileus, consisting of merely an inverted peristaltic action of the intestines, from irritation, and unaccompanied by obstruction, powerful antispasmodics, as opium, æther, and effervescing draughts, with sedative glysters, are evidently indicated; and in the very few instances of simple ileus which we have met, this treatment has been quickly efficacious. But as this form is not only rare, but liable to be confounded with, or quickly terminate in, those forms in which there is obstruction, we dare not safely allow our treatment to proceed upon the purely antispasmodic plan. The very same remark will apply to those forms of the disease in which there is

only spasmodic obstruction to all appearance. The transition from spasm to inflammation, volvulus, intus-susception, &c. is often so sudden, and the difficulty of distinguishing between it and them so great, that we should never lose sight of these last, in our treatment.

We believe then, that whenever there is *pain* in any part of the abdomen, we should bleed, both generally and locally, while we exhibit a combination of sedatives and cathartics. Two or more grains of opium, we have usually combined with ten grains of calomel, and as many of compound extract of colocynth, to be taken at once. Large emollient glysters, with laudanum and salts, are to be thrown up, while the warm bath is preparing. Fomentations to the abdomen, after leeches, may precede large blisters, which are useful auxiliaries, though too often neglected.

When Dr. Abercrombie comes to the treatment of ileus, he appears so stricken with the inapplicability of his theoretical principles, that, with an ingenuousness characteristic of great minds, he acknowledges that he "does not presume to think them so established as to be applied to the treatment of ileus."* But surely that theory is exceedingly fallacious which is not drawn from the best *methodus medendi*, as well as from symptoms and dissections. We have always made it a rule to instantly reject the hypothesis which ran counter to any one of the three data above mentioned.

Let us apply Dr. Abercrombie's "remedy of most general utility, the tobacco injection," to his theory of ileus. "The obvious effect of this remedy, says Dr. A. upon the system is to produce *relaxation of all muscular parts*, and the mode of its operation in ileus is involved in considerable obscurity." Why, yes. If Ileus depend on a paralytic or over-distention of a portion of intestine, and this weakened and paralysed part be restored suddenly to healthy action by a remedy which "produces relaxation of *all muscular parts*," then "chaos is come again," and it is in vain to ever reason upon a medical subject in future! But let us apply the *modus operandi* of the tobacco infusion to the simple, and we trust, natural view of the pathology of ileus, which we have taken throughout this paper, and all this

* Edinburgh Journal, p. 23.

obscurity vanishes; the mind finds a resting place amid the wilderness of conjecture, and feels that pleasing sensation which always results from the "*rerum cognoscere causas*."

We have endeavoured to shew that the *contracted* portion of intestine, however induced, below the distended portion, was the *fons et origo mali*; and what can be more natural than that a medicine which relaxed *all* muscular parts, should operate beneficially here? The thing is so self-evident, that we will not insult the understanding of our intelligent readers, by saying a word in explanation of the principle. Who can fail to perceive that Dr. Abercrombie, in the following passage, by confounding "*tonic power*" with spasmodic or other morbid contraction of the gut, has led himself into all this labyrinth of obscurity?

"I have supposed (says he) that, in this disease, the upper part of the canal is healthy, sometimes in strong action; that a part below this is inactive from distention, and that the lower part is *healthy and contracted*, being kept in that contracted state by its *tonic power*, and the suspension of the action by which, in the healthy state of the parts, it would have been distended. A certain force is, indeed, acting upon it by the propulsion of matters from the upper part, but this acts with little effect after being communicated through the intermediate portion, which is in the state of an inanimate canal. It is, therefore, unable to overcome the *tonic contraction* of the lower part, *which thus opposes an obstacle* to the parts recovering their healthy relations. The same observation applies, if we suppose that the distended part itself retains some degree of action, though feeble and imperfect. Now, in this state of the parts, could the tonic power of the lower part be, for a time, considerably diminished, it might perhaps be brought, as it were, more into unison with the other parts; might be dilated in the natural manner by the weakened force which is acting upon it; and the parts might thus be enabled to recover their healthy relations. Is this the action of the tobacco injection?"

If it be, it is a marvellously accommodating action. It relaxes the fibres of one portion of intestine; (one would almost suppose in compliment to Dr. Abercrombie's hypothesis,) but there it sud-

denly stops, and produces no relaxation in the portion of intestine above this! Thus too, the *constricted* part of intestine has nothing to do with the *pathology* of ileus; it was, in fact, in a state of perfect health and ease. But when we come to the treatment, this comparative exuberance of health stands in our way, and we are forced to apply the infusion of tobacco to dissipate it! In other words, Dr. Abercrombie first laboured to prove that the *contraction* of the gut below, was the *consequence* of "the suspension of action from above," (p. 7) and now he labours to prove that by taking away this *effect* the cause of it will cease! Can Dr. Abercrombie reflect upon these things, and not see, at once, that he has confounded cause and effect, and that he has been led into erroneous reasoning by mistaking morbid constriction for healthy tonicity?

We have paid Dr. Abercrombie a great compliment by investigating so narrowly his doctrines, on this point, which are calculated to lead to the most destructive practice. One of our contemporaries, (of very superior discernment,) enamoured of the paralytic doctrine in ileus, wonders that Dr. Abercrombie did not suggest *wine* or other stimulants as the radical treatment, which would aim, at once, at the origin of the disease!

We agree with our author in his *practical* precepts. He very properly warns us against active purgation, when there is already "a strong, though ineffectual effort to overcome some interruption to the healthy action of the canal." It is on this principle that we always combine a sedative with the cathartic; and that we employ blood-letting, not only as an antiphlogistic, but as an *antispasmodic* remedy of paramount importance. Dr. A. observes "that a full dose of opium is sometimes followed by the result which we have sought for in vain from the most powerful purgatives;" a fact, by the bye, which we should conceive is much more in favour of a spasmodic than a paralytic cause of the disease. The following passage respecting tobacco injection, we gladly quote on such respectable authority, though the remedy has been employed for a very long time in ileus.

"It should be begun with caution; perhaps for an adult, in the quantity of fifteen or twenty grains infused in four or six ounces

of hot water. After the interval of an hour or two, it may be repeated in a little larger quantity, until such effects are produced by it, viz. slight giddiness and muscular relaxation, as show that it is exerting its proper effect upon the system. It may then be repeated, at proper intervals, a great many times, if the case do not yield. With these precautions, I have given it in states of great nausea and exhaustion, with the effect of diminishing instead of increasing them; and, in one case, to a child three years of age, with the happiest result. In one of the most severe and obstinate cases I have had occasion to treat, it was repeated nearly twenty times with various partial effects, and at length with complete success."

We have not had occasion to try the effects of cold water externally, or by glyster, as mentioned by Dr. Abercrombie, but we confess we would be more inclined to employ the warm fomentations and enemata.*

In the first number of this Journal, page 133, Dr. Porter, of Bristol, relates an interesting case of ileus, where stercoraceous vomiting had taken place to a great extent, and where five grains of solid opium were exhibited, a large blister applied, and a cold saline enema thrown up. These stopped the vomiting, but did not procure stools. Ten grains of calomel were next ordered, and small doses of the oleum terebinthinæ every hour. On the succeeding day, the fæces appeared in abundance, and recovery was complete.

Sydenham's practice in ileus was as inert as his theory was erroneous; but his translator, Dr. Swan, makes some very judicious remarks on the treatment of this complaint. "A proper quantity of calomel, (says he) made up into a pill, will more certainly pass than any thing else; and for fear that, in a dose of about twelve grains it should irritate the stomach too much, it may be given in a

* "Home d'Edinbourg, assure que l'ether sulfurique à l'interieur, combiné avec les pediluves d'eau froide, lui a parfaitement bien reussi. De Haen dit s'être bien trouvé des lavemens excitans avec la fumée de tabac." *Mont-falcon.*

There are some instances on record, where spasmodic colic has been resolved by dashing cold water on the lower extremities, or walking on a cold floor.—REV.

less quantity, and repeated as there is occasion; and an opiate may occasionally be mixed with this or other pills. And as the most plentiful bleedings ought to be used, and fomentations frequently applied in this case, there seems to be little danger of inflaming by calomel."

We may here remark that the vomiting in ileus has always appeared to us to be a salutary effort of Nature to remove by the mouth what could not pass downwards, as well as to determine to the skin, and obviate inflammation. On this account, we have never been solicitous to stop the vomiting, except by removing the cause of it. Barthez is in the habit of ordering leeches to the anus, while glysters of herby decoctions, with salts and laudanum, are thrown up into the intestines. He gives asafoetida and camphor, in small but repeated doses, by the mouth. Quarin speaks favourably of a solution of tartrite of antimony in glyster, which acts, probably, on the same principle as the tobacco enema.

Where intus-susception is suspected, it has been suggested to inject large quantities of liquids into the intestines, per anum, with the view of disentangling the obstruction;* and Mr. A. Blacklock, of Dumfries, from experiments on the dead body, asserts that air thrown up by a pair of bellows, is exceedingly powerful in removing intus-susceptions. We need hardly remark that the practice of swallowing quicksilver, or leaden bullets, is totally unwarrantable; and the interesting case of gastrotomy, which we have detailed in the second number of this Series, page 255, affords little prospect of success from a surgical operation in such desperate circumstances. The cases of Mr. Smith, in the ninth vol. of the Ed. Journal, treated partly by *cold water*, do not appear to us at all in point; and notwithstanding the praises bestowed on this remedy by Alexander of Tralles, Septal, and Hoffman, we should be inclined to believe with Monfalcon, that "*La méthode réfrigérante, dans le traitement de l'iléus, a été entièrement abandonnée et devoit l'être.*"

This article has extended much farther than we expected; but

* See Mem. Med. Society of London, vol. ii.

we hope that we have not taken up our own, or our readers' time unprofitably. If it contain aught of merit, it is attributable to Dr. Abercrombie, whose interesting paper excited our attention to the subject, and whose other papers will call forth similar inquiries. We have examined Dr. A's doctrines with a critical closeness proportioned to the esteem in which we hold him, and the honour which we mean to pay him in our future investigations and strictures. We have plenty of subjects for the exercise of criticism; but we have no desire to measure our strength with any but the strong.

X. Peritonitis.* Acute rheumatism, while it retains its seat in the tissues of the extremities, is a painful, but not a dangerous disease. When a metastasis to an internal structure takes place, the danger is great; and the patient is never safe till the original affection is restored, or at least till the metastatic action is completely subdued. The serous membranes of the brain, heart, and lungs, are those which generally suffer in translated rheumatism. The abdominal viscera are more rarely affected. The following case, however, shows that they are not exempt from these dangerous visitations of a migratory disease.

Case. Bridget Daly, ætat. 28, was admitted into hospital on the 22d of May, labouring under acute rheumatism, and complaining of excruciating pain in her knees, which were tumefied, in consequence of effusion into the articulations. The slightest motion was intolerable; she had thirst; was restless and anxious; pulse quick and full; temperature of surface increased; bowels constipated.

This attack commenced about three weeks after parturition, with rigors, heat, and thirst. In a day or two after its commencement, she was affected with general soreness, followed by pain and swelling of the knees. V. S. on the 23d and 24th. Purgatives. Prescribed a pill of calomel, antimony, and opium, 4ta quaque hora.

* Case of Acute Rheumatic Inflammation, terminating in Peritonitis. By E. McDowell, Surgeon. Dublin Hospital Reports, vol. ii.

25th. Fomentations to the knees, and an anodyne draught ordered.—26th. A copious perspiration had not brought relief. To omit the pills, and take decoct. cinchon. with ant. tart. every four hours. Intense pain in the knees, which are more swelled. Œdema of the right leg was observed to day. Twelve leeches applied; tepid bath.—28th. Pains greatly relieved; swelling of the joints diminished; tepid bath repeated at her own desire.

On the 2d of June, an alarming change for the worse. She was found sitting up in bed, leaning forward, seemingly in great agony, the abdomen being very hard, tense, and painful on the slightest touch; pulse quick, small, and very feeble. On inquiry, it was learnt, that on the 27th she had been affected with pain in the bowels and griping, which were removed by a dose of castor oil. She was bled immediately, and eighteen leeches were applied to the abdomen, followed by fomentations and glysters, with small doses of neutral salts. Blood buffed and cupped. The disease seemed to remit in severity after the bleeding; but the abdomen was still tender. At night the symptoms of peritoneal inflammation recurred, and she sunk in a few hours.

Dissection. Abdomen tumid; small intestines much inflated, large intestines contracted. Peritoneum highly inflamed, being uniformly coated with coagulable lymph of a dusky hue. On raising it, the subjacent membrane was found minutely vascular, and of a bright red colour. Considerable effusion of serum in the abdomen, with flakes of lymph floating in it. The peritoneal tissues of the uterus and ovaria, covered with a thick layer of lymph. The *mucous* membrane of the stomach showed marks of inflammation, but that of the intestines was healthy.

In the knee joints, the whole of the synovial membrane internally was covered with lymph, closely adherent to it; in each articulation about two ounces of fluid, resembling that found in the abdomen. Bands, formed of lymph, stretched across the articulation, in several places. The surrounding fibrous structure was unaffected. The cartilages and bones sound.

We are convinced, from many circumstances which have fallen within the range of our observation and reading, that the warm

bath is a dangerous remedial measure in most *acute* diseases; but especially in acute rheumatism. When there is what has been called a determination to any *external* part of the system, the warm bath must have some tendency to increase that determination. But this is not the danger to be apprehended. It is the subsequent *recoil* from the surface to the interior which is to be dreaded, and which may establish a point of irritation and inflammation there, that absorbs the preceding disease, or invites its translation to itself.

XI. *Lithotomy*.* The other great operations in surgery have undergone the most important changes in our own times, whilst on lithotomy, surgeons have been chiefly occupied in bickerings about the preference of this or that form of instrument for the performance of the operation. An Alanson, an Abernethy, a Cooper, a Lawrence, and a Thomson, have carried the operations of amputation, of aneurism, and of hernia, to a degree of perfection unknown to our predecessors in the healing art; while the operation of lithotomy remains unchanged, after all that has been written on it, since the time of our celebrated Chesselden. On this account we read with unusual interest and pleasure the new method of operating, adopted by the Italian surgeon, which, from its novelty and importance, we think deserving of a full notice. The subject of Professor Barbantini's operation was a countryman of about 50 years of age, whose constitution had suffered considerably from the long continued irritation of his distressing disease. On examination, the calculus was discovered to be very large, bearing down by its weight the fundus of the bladder, so as to form a tumour projecting into the rectum, and occupying the space from one tuberosity of the ischium to the other. This calculus being evidently too large for extraction by the lateral operation, and knowing well the difficulty and danger of breaking down the stone in the bladder, Professor B. gave up all idea of it. The high operation still remained; but he considered that

* Account of the extraction of a large Calculus from the urinary bladder through the rectum. By Dr. N. BARBANTINI, Professor of Clinical and Operative Surgery in the Royal Lyceum, and Surgeon of the Hospital of Lucca. [From our Correspondent at Rome.]

method as affording him little hope of success. "L'Esito troppo spesso infelice di questo modo di operare, me lo aveva fatto sempre riguardare come uno di quei mezzi, che l'arte salutare adopera con avveduto consiglio, perchè niun altro ne conosce men arduo, o men pericoloso, e che la necessit   sola consiglia." The extraction, by the rectum, was then determined on. Having first introduced a sound into the bladder, he divided the sphincter ani, and integuments of the perineum, to the extent of about half an inch from the anus; introducing his finger into the wound, he now felt the sound, and guiding his scalpel by the finger, made a small incision into the bladder, cutting upon the sound a few lines beyond the prostate; into this incision he introduced a long director which he insinuated between the stone and fundus of the bladder, on which it rested, and on it divided that viscus to an extent sufficient to admit the passage of the stone. The parts divided in this operation were a few fibres of the transversus perinei muscle, the whole of the sphincter ani, on its anterior part, the same part of the rectum, and the anterior inferior part of the fundus of the bladder; the quantity of blood lost was very trifling. The extraction, from the great size of the stone, was attended with some difficulty, and required much more force than is necessary on ordinary occasions. The stone, which was something of an oval form, weighed nine and a half ounces; its length (measuring from the engraving now before us) three and a half inches; its greatest breadth two inches and five-eighths.

A slight degree of fever occurred on the second day after the operation, but soon disappeared, and no untoward symptom occurred during the progress of the cure. In dressing the wound, the sphincter ani was kept from adhering too quickly, that the f  ces might have a free exit, and run no risk of being forced into the bladder during their evacuation. For the first eighteen days the urine passed entirely by the anus, with the exception of a few drops only; which, from time to time, escaped by the urethra. On examining, at the end of this time, the parietes of the bladder were found thickened and hard, and to its internal surface some gravelly particles were found strongly adherent; these were separated by the finger, together with several portions of a new formation of a firm membranous substance (*pseu-*

do-membrana molto consistente) and this operation repeated till these substances could no more be felt. An elastic catheter was now introduced through the urethra into the bladder, and by it the urine passed almost entirely; when the fæces were fluid only a small quantity entered the bladder, passing off with the urine by the catheter, and producing no inconvenience. At the end of fifty days from the operation the patient felt so little inconvenience, that he requested to return home. The wound, on examination, was then found diminished to half its original size, the healing process having taken place in the posterior part of the wound. The thickening and induration of the bladder had also nearly disappeared. The catheter was now laid aside, and at the end of thirty days more the patient wrote that he was entirely well; that he passed his urine at pleasure, and no more passed by the rectum. We have thus given a pretty full account of Professor B's case, which he has related plainly and accurately, because we think it a valuable addition to our stock of surgical knowledge, as shewing (proving) that there is a safer and easier method than any before practised, of extracting large calculi from the bladder. We have even our doubts whether from the facility with which this operation may be performed, the absence of all risk of hæmorrhage, the ready exit which it admits of to the stone, and consequently the much less hazard that it presents of being followed by the dangerous consequences resulting from laceration of the parts during extraction, it may not, upon trial, be found preferable to the lateral operation in a great majority of cases. The readiness with which the urine flowed by the catheter, when introduced at the end of eighteen days, is very satisfactory, as at that period the wound of the bladder remained of its *original* size; and hence we may conclude, that had the catheter been introduced sooner, the same thing would have happened. We can see no objection to its being introduced from the day of the operation; on the contrary, its being so may facilitate the healing of the wound, by removing the irritation arising from the passage of the urine by it. But our limits forbid us to enter farther on this subject, convinced that it is sufficient to have made our countrymen acquainted with the case, to call their attention to it. With regard to the origin of the recto-

vesical operation for the stone, our author observes, that Haller, in his *Bibliotheca Chirurgica*, mentions an Italian surgeon of the name of Végèce who advised it. *Jubet per vulnus recti intestini vesicæ, aculeo lapidem ejicere*; and lately, Dr. Sanson (in the *Dictionnaire des Sciences Medicales*, Art. Lithotomie) also recommends it; but it has been first put to the test of experience in that country where the idea of its practicability first originated. Professor Barbantini certainly deserves the highest credit, and the thanks of mankind in general, for shewing a method of relieving them from one of the most painful and harassing diseases to which they are obnoxious; and if, by thus giving circulation to his case, we shall be the means of relieving a single individual that would otherwise have been carried by his malady to the grave, it will be a cause of infinite gratification to us.*

XII. *Anteversio Uteri.*† In this little volume of Dr. Granville's we have observed a great many interesting cases, and are sorry we cannot devote space for a general review of the work. We shall, from time to time, however, introduce particular cases, under their appropriate heads, which will afford sufficient indication of the merits of the publication.

Every displacement from the natural position of the uterus demands serious attention; and we fear that manual examinations are not so frequently made as they ought to be, from false delicacy on one side, if not on both. Eliz. Stedman, ætat. 28, of very nervous temperament, had aborted three times, at eight and ten weeks. She became weakly in consequence, and very irritable. In this state she had continued several years before she came under the

* The high operation was lately performed in the Santo Spirito Hospital, at Rome, on a very unfavourable subject, with complete success. CORRESPONDENT.

Our Correspondent had a conversation with Mr. John Bell at Rome, relative to the above operation, and informs us that the latter "thinks very well of it." EDIT.

† A Report of the Practice of Midwifery, at the Westminster General Dispensary, during 1818, &c. &c. &c. By AUGUSTUS BOZZI GRANVILLE, M. D. F. R. S. F. L. S. M. R. I. Physician in Ordinary to His Royal Highness the Duke of Clarence; Licentiate of the Royal College of Physicians, &c. Octavo, pp. 220, 1819.

care of the Reporter, who found her affected with hysteria, impaired digestion, and suspended menstruation. These complaints were, in a great degree, removed by appropriate remedies; but still she suffered excruciating pains in the back, great heat and irritation in making water, with sense of weight immediately behind the pubis, bearing down, and inability to retain her urine. These symptoms were accompanied by a thin serous discharge, which excoriated the parts. On examination, Dr. Granville found the vagina relaxed; the urinary bladder weighed down, and pressed against the concave surface of the pubis; the os uteri turned upwards and backwards, while its fundus had fallen forward, and rested upon the bladder. Dr. G. endeavoured to rectify this mal-position of the parts manually, and succeeded in giving strength to the parietes of the vagina, and checking the discharges by the cold bath, and the following vaginal injection which the Doctor recommends. R. Decoct. quercus 3x; decoct. ros. caninæ 3vj; zinci sulph 3j; super-sulph. aluminis 3iß; alcohol. 3ij; infus. opii aquos. 3ß; m. ft. injectio.

What contributed most to the poor woman's recovery, however, was, "an improved bandage in the shape of stays, by the help of which the abdominal muscles were supported, and all the viscera retained *in situ*. The patient became pregnant.

XVI. *Eczema Rubrum*.* A female, aged 34, was admitted into the hospital, with a copious discharge of thin yellowish foetid ichor, from almost the whole surface of the body, the cuticle scaling off, leaving the surface beneath of a red colour. Appetite good; pulse 110; tongue clean, bowels loose; catamenia irregular. She had been taking pills for a cough, about three months before, supposed to be mercurial, though they never affected her mouth, and during that period caught cold. The eruption then broke out. The ungt. cetacei applied to the parts—decoct. cinchonæ internally; the tepid bath. Subsequently, the cerat. plumb. super. was applied, and the sulphuric acid prescribed internally. Under this treatment, she recovered in about three or four months,

* Case of Eczema Rubrum, with Remarks. By Dr. Crawford, Jun. M. D. of the Hampshire County Hospital. *Ed. Journal*, 62.

but relapsed as bad as ever, all on a sudden. An ulcer on her leg had been dressed with ungt. nitrat. with some of which she had secretly anointed the axillæ, which were a little hard and rough. On closely observing the eruption this time, it had the appearance of innumerable minute vesicles, which, after two or three days, discharged a thin yellowish ichor, eroding the skin, and reducing the patient to the state before described. Linimentum aquæ calcis to the parts; citric acid internally; and when the irritation was abated, the tepid bath. In sixteen days she was well. Soon after this a dose of opium reproduced the disease. Dr. Moriarty mentions a case in which the eruption was also reproduced by opium three or four times.

Dr. Crawford justly doubts the effects of the *internal* medicines in this case. The last relapse was of shortest duration, although nothing but the tepid bath was employed. This eruption, especially in its primary attacks, appears most commonly to be owing to the action of mercury on a peculiar idiosyncrasy of constitution. Relapses often occur from other medicinal substances. "As this eruption puts on a *vesicular* form, the appellations of mercurial erythema or lepra, are inapplicable to it; and as mercury is not the only exciting cause, it is improper to denominate it hydrargyria or eczema mercuriale."*

§ VII. NERVOUS SYSTEM.

XVII. *Chorea*.† Chorea appears to be a disease engrafted on the human constitution, like many other of the neuroses, by modern refinement and civilization. The *scelotyrbe* which Pliny (*lib. xxv. c. iii.*) describes as affecting the army of Germanicus on

* We are happy to find that a Dispensary for Diseases of the Skin has been instituted in this metropolis, under most respectable auspices, and with the advantages of such able medical officers as Dr. Thompson, Dr. Emery, Mr. Wadd, Mr. Carpue, &c. Such an institution deserves the patronage of the profession, and the gratitude of the public.

† On the Use of Arsenic in the Cure of Chorea. By Mr. Salter, Surgeon of Poole.—*Medico-Chirurgical Transactions*, vol. x.

the Rhine, and which he attributes to unwholesome water used for drink, was supposed by Sauvages to be the disease in question, but the analogy of the two affections is too loose to warrant the conclusion drawn. It was not till the close of the sixteenth century, in fact, that chorea was correctly described, by Plater, Horstius, and Sennert. It is now well known, for it is far from being uncommon, though its pathology and treatment are by no means settled. We are of opinion that the neglect of viewing it under its idiopathic and symptomatic characters distinctively, has contributed to the diversity of sentiment respecting its nature and treatment. The idiopathic chorea is that form of the disease which is neither the symptom nor the effect of any other malady, and which generally appears about the epoch of puberty. Its predisposing causes are, the revolution in the system at the period alluded to, in subjects of feeble constitution from infancy, and whose parents have been of nervous and irritable dispositions. Its exciting causes may very frequently be traced to mental agitations, as frights, violent fits of anger, the depressing passions, and certain affections of the genital system.

The symptomatic chorea offering nearly the same phenomena as the idiopathic, is dependent on a variety of local affections which should be carefully investigated; as too great fulness of the cerebral vessels; or the result of injuries of the head, irritation in some portion of the primæ viæ, or organs connected with digestion, suppressed eruptions, or general derangement of the nervous system, from various mental or physical causes.

Any person who takes the trouble to reflect on these circumstances in the etiology of the complaint, will readily perceive that no one method of cure can be successful in all, or even in a majority of cases. Thus, when chorea appears as an idiopathic affection about the epoch of puberty in a plethoric girl, the practice of Sydenham, repeated bleeding and purging, will be the most efficacious treatment. If the patient be of a delicate, irritable, and what is termed a nervous disposition, then tonics, especially the oxides of zinc and bismuth, or arsenic, with the fœtid gums and eccoprotics, will be the treatment indicated. If chorea depends on irritation or mal-secretion in the line of the digestive

organs, a continued train of purgatives, on the plan of Dr. Hamilton, will be useful. We may here state, however, what we believe a great number of practitioners could substantiate, that purgatives are not even *generally* successful in the complaint under consideration, when trusted to alone. We have been so often disappointed, in following up Dr. Hamilton's directions, that we have long since laid them aside, except as auxiliaries. From the oxide of zinc, indeed, with alteratives and antispasmodics, we have derived, upon the whole, the most satisfactory results. But the etiology of the disease, as was before observed, should be particularly sought after, and we can confidently assure the practitioner, that the labour of his investigation will be amply repaid in the facility and rapidity of the cure. After this short, but we hope not irrelevant preliminary, we shall come at once to the subject of this article.

Soon after reading the account of a case of chorea cured by arsenic, in the 4th vol. of the Medico-Chirurgical Transactions, Mr. Salter had an opportunity of treating a case of the disease. He was unsuccessful with the purgative plan, and therefore "was induced to make trial of the liquor arsenicalis, and had soon the happiness to find his patient rapidly recover." In this case, however, he used the pil. gambog. compos. and, therefore, the advocates of purgation may attribute a great share of the success to the purgative medicine. But in the three cases detailed in the volume before us, "no other medicine was used than the liquor arsenicalis, nor was any change of regimen allowed; its efficacy, therefore, in them, must be considered as incontrovertibly established."

CASE 1. Eliz. Hardman, ætat. 17, dark complexion, eyes, and hair, had chorea for three months. The members and trunk were affected; and indeed, the whole body was frequently thrown into the most grotesque attitudes. The bowels were constipated; the catamenia regular. After four days of purging, the complaint was rather increased than meliorated.

Four minims of the liq. arsenicalis were then ordered thrice a day. In two days the involuntary motions were somewhat less

violent. The dose to be increased by one drop each time per diem. In ten days more, the patient was so much better that she appeared to be rapidly getting well. Fourteen drops of the liq. ars. were now taken thrice a day, the dose not to be increased. In a fortnight, this treatment removed the chorea, and greatly improved the general state of the patient's health. Griffith's mixture, with pil. aloes, cum myrrh, completed the cure.

CASE 2. Miss P——, a delicate girl, of nine years, became affected with chorea, immediately after being frightened by one of her school-fellows. The free use of the purgatives, with the decoct. cinchonæ, was tried for nearly three weeks, without the least success; on the contrary, the symptoms were daily augmenting in severity. The function of speech was nearly suspended. She now commenced with three drops of the liq. arsen. thrice a day. In a month of this treatment, a striking impression had been made on the complaint, and in a few days more, the chorea disappeared entirely. A slight relapse was again cured by the arsenic.

CASE 3. Mary Brown, ætat. 12, delicate constitution, had long been affected with chorea; but, during the last six weeks, it had been violent. She began with three drops thrice a day, increasing the dose one drop every day, until as much is taken as the stomach will bear. In three days she was better, and in less than a month she was cured.

These cases are interesting. We think that Mr. Salter was somewhat bold in the first case. Fourteen drops thrice a day, for more than a fortnight, is more than we would like to exhibit of the liq. arsenicalis, though few have a more favourable opinion of the medicine than we have. We consider it, in fact, the most powerful tonic in the Pharmacopœia, and we have given it in a very great variety of chronic and anomalous affections, with the most decided benefit. We have not seen any injurious effects result from its use, when not carried farther than eight or ten drops thrice a day, and never continued longer than a fortnight at one time. But we have seen a few, and heard of several cases, where a longer continuance of the medicine was injurious to the stomach.

§ VIII. GENERAL SYSTEM.

XVIII. *Tetanus*.* This most formidable disease has been too justly termed the opprobrium medicorum. It is, indeed, one of those which often reduce the practitioner to a painful dilemma; for he must either incur the risk of killing his patient, or see him carried off amidst the most unrelenting and frightful tortures.

The following case, in which it was successfully treated in its most dangerous form, affords decided evidence of Dr. Stewart's practical skill, and leads us to expect results equally beneficial to his patients and creditable to himself, from the cultivation of other departments of that extended field in which he is now exerting his ingenuity and his talents. This account, which is merely extracted from a private letter, contains only the leading features of the case; but these, as well as the judicious means by which the cure was accomplished, are so clearly, though briefly described, as to merit the attention of the medical public.

Case. The patient was brought to the public hospital at Cape Henry, with a large splinter of wood in the sole of his foot; his jaw considerably locked; the muscles on the back of his neck and trunk so strongly contracted as to form the spine into a complete arch, and his extremities perfectly rigid. After removing the splinter, Dr. Stewart began the treatment by giving two ounces of the oleum terebinthinæ, mixed with one ounce of castor oil, twice a day. This plan was continued for five days; during which, glysters, containing each an ounce of the oleum terebinthinæ, were exhibited three times a day. During the first day, two grains of opium were given every three hours; during the second day, two grains every two hours; during the third day, two grains every hour; and during the fourth, fifth, sixth, and seventh days, the same dose was repeated every half hour. The patient took two bottles of Claret daily. A large blister was applied to the pit of the stomach on account of local pain. By the oleum terebinthinæ and castor oil, he was freely purged every day; and

* A Case of Traumatic Tetanus, successfully treated. By Duncan Stewart, M. D. Surgeon in Chief to the King of Hayti; with Remarks by J. Allan, Esq. Surgeon, London.

operation which, as Dr. Stewart remarked, was promoted by the opium. After the fifth day, the fits of spasm became less frequent; and, for the first time after his admittance, the patient enjoyed some sleep. After that period too, the opium was the only medicine given, and yet the purging continued regularly, and was, by Dr. Stewart, ascribed unequivocally to that medicine. When a decided amendment of the symptoms had become apparent, the doses of it were gradually decreased. On the tenth day, the patient was quite free from the disease; and what is very remarkable, one grain of opium twice a day *then* occasioned headach, thirst, and giddiness. During the process of cure, the wound was dressed with simple poultices.

Dr. Stewart adds, that several less severe cases had yielded more readily to the same plan of treatment; and it is by no means intended here to anticipate any observations on this disease which he may probably hereafter communicate to the public; but it may be useful at present to compare with the plan adopted by Dr. Stewart the observations contained in the following extract from the Treatise of Dr. Morrison on this subject, as it appears at page 375, in Dr. Johnson's work on Tropical Climates. On this comparison, it will be found, that in resorting to the oleum terebinthinæ, and freely purging the bowels, Dr. Stewart effectually accomplished what Dr. Morrison considers a great desideratum, but exceedingly difficult to be effected. The remark made by Dr. Stewart relative to the operation of opium, in promoting catharsis, is also conformable with Dr. Morrison's experience.

"The bowels (says this gentleman) should be kept as free as possible. We must endeavour to bring about an operation every twelve hours. This, even by the aid of strong cathartics, or purgative injections, will be found very difficult to be obtained, the sphincter ani sometimes scarcely admitting the introduction of a glyster-pipe, and the exhibition of the strongest purgatives may often be attended with little or no effect. Sulphate of soda, jalap and calomel, scammony, pil. aloës, cum colocynthide, &c. are as proper for this purpose as any other, aided by stimulating glysters, such as solutions of muriate or sulphate of soda, with olive oil, the resin of turpentine suspended by the yolk of an egg, solutions of soap, &c. &c. I have found it, on two or three occasions, impos-

sible to open the bowels freely, till after large quantities of opium had been taken, which seemed to bring about a general relaxation." Dr. Morrison once gave a patient, who ultimately recovered, ten grains of opium and twenty of calomel in pills, and five ounces of tincture of opium in wine, within the space of twelve hours.

"As next in importance to opium, Dr. Morrison recommends the preparations of mercury in large doses internally, conjoined with a free use of frictions with the ointment externally, and with these he recommends that liberal, and, indeed, great quantities of wine and ardent spirits should be given."

Dr. Johnson states his opinion, that "tetanus is radically an inflammatory disease," and the writer of this article could confirm the accuracy of this opinion, by the detail of a case in which the disease was cured by bleedings, repeated to the extent of above one hundred and thirty ounces, active purging with senna and salts, and the application of blisters. Nevertheless, Dr. Johnson justly remarks,* "that the local abstraction of blood by leeches, and cupping from the neighbourhood of the spine, with subsequent blisters there, are not inconsistent with the plan of treatment recommended by Dr. Morrison. For it must be remembered, that such is the unequal distribution both of the blood and excitability in the system, under this disease, that one part is completely torpid, while another is on the point of extravasation from turgescence or inflammation. It is evident, from this view of the affair, that we must stimulate the torpid organs at the very moment we are employing sedatives and counter-irritants, or abstracting blood from the congested parts. Hence, too, the great value of purgatives and mercury. The former bring back the excitement to the abdominal viscera, and powerfully determine from the spine: the latter sets all the secretory and excretory apparatus to work, while it equalizes the circulation in every part of the system.

The still more simple plan of treatment acted upon by Dr. Stewert, as far as it has been tried, seems, without the aid of mercury, to promise the accomplishment of all the intentions proposed by Drs. Morrison and Johnson, merely by the well directed and vigorous application of three well known medicines, oleum terebinthinæ, opium, and castor oil.